THE
ASTROPHYSICAL JOURNAL
AN INTERNATIONAL REVIEW OF SPECTROSCOPY
AND ASTRONOMICAL PHYSICS

VOLUME XVI OCTOBER 1902 NUMBER 3

ON THE SEPARATION OF CORRESPONDING SERIES LINES IN THE MAGNETIC FIELD.[1]

SECOND PAPER.

By C. RUNGE and F. PASCHEN.

IN our paper on the radiation of mercury in the magnetic field,[2] and in the first paper on the separation of corresponding series lines in the magnetic field,[3] we have discussed the so-called triplet series, which were discovered by Kayser and Runge, and by Rydberg in the spectra of *Mg*, *Ca*, *Sr*, *Zn*, *Cd*, and *Hg*. In addition to these series there have been found in the spectra of the alkalies as well as of *Cu*, *Ag*, *Al*, *In*, and *Tl* so-called doublet series.[4] It became interesting to investigate whether the same relations as to the separation in the magnetic field held good for these in the manner we have observed in the case of the triplet series.

As regards the alkalies, we did not, indeed, succeed in obtaining a separation of any lines whatever, with the exception of the

[1] Translated from *Sitzungsberichte der K. Akad.*, Berlin; Session of June 26, 1902.

[2] ASTROPHYSICAL JOURNAL, 15, 235, 1902.

[3] *Ibid.*, 15, 333, 1902. [4] See Kayser's *Handbuch der Spectroscopie*, II, 503-573.

D line, which had already been investigated by Cornu in the magnetic field. The other sodium lines were so lacking in sharpness in the spark spectrum that we could not recognize the type of separation in the magnetic field. No better result was found in case of lithium. We confined our observations to the D lines, and to those of *Cu*, *Ag*, *Al*, and *Tl*.

No principal series was found for *Al* and *Tl*, though for *Cu* and *Ag* the principal series is known, although represented by only one member. It appeared that the principal series of *Cu* and *Ag* exhibited precisely the same types as sodium in the magnetic field. And not only is the character of the separation the same, but when represented on the scale of vibration numbers the separation of the components is exactly the same with the same field-strength as in the case of the components of the D lines. We give in the following table the measurements of the distances of the components for the principal series of *Na*, *Cu*, and *Ag*. The measurements are reduced to the same field-strength, as the photographic plates were not made with exactly the same field-strength. We were careful to arrange so that on every plate, lines whose separation in the magnetic field had already been observed by us were also photographed. The lines thus simultaneously photographed were preferably the lines of the second subordinate series of triplets in the spectrum of *Zn*, *Cd*, or *Mg*, the separation of which we discussed in our first paper. These lines are admirably adapted for fixing the field-strength, as their separation is strong, and as the lines can be photographed very sharply with suitable self-induction in the secondary circuit. The two *Zn* and *Cd* lines, for which Mr. Faerber has investigated at the physical laboratory of the University of Tübingen the dependence of the separation upon the field-strength, belong also with these lines. If we make use of his measurements, which he kindly placed at our disposal, the field-strength to which all the measurements in this paper are reduced, is found to be equal to 31000 c. g. s. units. This should deviate from the truth by less than 1 per cent. according to Mr. Faerber's data. But if we depend upon the measurements of field-strength made by

Michelson, by Marchand and Blythswood, and by Reese, upon which we based our figures as to the field-strength in our paper on the radiation of mercury in the magnetic field, and to which our mercury measurements are referred, then we obtain a field-strength of 32000 units for the measurements in the present paper. The value 24600, which we then gave for the field-strength for the mercury lines, would be only 23850 units according to Faerber's measurements. We believe his determinations to be the more reliable.

The numbers in the following table, which contains the separation of the pairs of the principal series in the spectra of *Na*, *Cu*, and *Ag*, for a field-strength of 21000 units, indicate the separations of the components measured from their center of gravity on the scale of vibration numbers (number of vibrations in a path of one centimeter). Positive numbers signify distances in the direction of larger vibration numbers.

PRINCIPAL SERIES.

Na λ5896	*Na*¹ λ5896	*Na*² λ5896	*Cu*² λ3274	*Cu*² λ3274	*Cu*¹ λ3274	*Ag*² λ3383	*Ag*¹ λ3383	Mean	Remarks
−1.88	.....	−1.84	−1.85	−1.88	.....	−1.86	.....	−1.86*s*	*s* denotes that vibrations were perpendicular to lines of force.
−0.90	−0.94	.....	.....	.....	−0.97	.....	−0.97	−0.93*p*	
+0.93	+0.94	.....	.....	.....	+0.97	.....	+0.97	+0.94*p*	
+1.85	.....	+1.84	+1.85	+1.88	.....	+1.86	.....	+1.85*s*	*p* that vibrations were parallel to lines of force.
Na λ5890	*Na*¹ λ5890	*Na*² λ5890	*Cu*² λ3248		*Cu*¹ λ3248	*Ag*² λ3281	*Ag*¹ λ3281		
−2.28	.....	−2.17	−2.34	.....	.....	−2.34	.....	−2.25*s*	
−1.43	.....	−1.38	−1.40	.....	.....	−1.39	.....	−1.40*s*	
−0.47	−0.45		.....		−0.40	.....	−0.45	−0.45*p*	
+0.46	+0.45	.....	.....	.....	+0.40	.....	+0.45	+0.45*p*	
+1.35	.....	+1.33	+1.38	.....	.....	+1.39	.....	+1.35*s*	
+2.36	.....	+2.22	+2.36	.....	.....	+2.34	.....	+2.30*s*	

For the plates designated by¹ the vibrations perpendicular to the lines of force, for those designated by² the vibrations parallel to the lines of force were cut out by a calc-spar. It is difficult to separate all of the components for the small wave-lengths of the *Cu* and *Ag* lines without this procedure.

The components (*p*) vibrating parallel to the lines of force are the strongest. The neighboring perpendicular vibrations

next follow. The extreme components, occurring for the smaller wave-lengths, are decidedly fainter.

The table shows that the separation of the three pairs of lines is identical within the accuracy of the observations. In taking the means triple weight has been given to the distances of the components of the sodium lines. The mean error of a measure of weight 1 is found from the deviations from the mean to be 0.056. On the scale of wave-lengths this corresponds to about 0.006 tenth-meters for the copper and silver lines. This can very well be ascribed to the inaccuracy of the observations.

The distances here exhibit a sort of law of multiple proportions similarly to the case of the components in the second subordinate triplet series. They are nearly equal to the even and odd multiples of a certain number.

	Distances of Components		Differences	
$2\times0.459=0.918$	-0.93	$+0.94$	0.01	0.02
$4\times0.459=1.836$	-1.86	$+1.85$	0.02	0.01
$1\times0.459=0.459$	-0.45	$+0.45$	0.01	0.01
$3\times0.459=1.377$	-1.40	$+1.35$	0.02	0.03
$5\times0.459=2.295$	-2.25	$+2.30$	0.04	0.01

The means of the above table have a mean error of 0.02, according to the deviations of the observations from the means. A mean error of 0.02 is also obtained from the differences of this table. Within the accuracy of the measurements, the distances therefore agree with the multiples of 0.459.

The distances of the components of the second subordinate triplet series are similarly multiples of a certain number, as we showed in our investigations of the radiation of mercury in the magnetic field and in our first paper on the separation of the series lines. For the same field-strength as that of the present measures that number would be 0.702. It is perhaps not due to chance but ultimately to a constant charge of the ions that these two numbers also bear a simple ratio to each other, for we have, nearly, $0.459:0.702=2:3$.

As was found by Rydberg, the principal series stands in a close relation to the second subordinate series, which among

other things necessitates that the lines of a pair of the second subordinate series correspond in inverse order to the lines of a pair of the principal series. It could therefore be foreseen that the separation in the magnetic field would bring to light the same relation, and this has been confirmed by the fact. The two lines of the pairs of the second subordinate series are separated in exactly the same way as the two lines of the pairs of the principal series, both in respect to the relative intensities and to the distances of the components; but the smaller wave-length for the one is separated just like the larger wave-length for the other, and *vice versa*. The same thing holds good for the second subordinate series of *Al* and *Tl*, although no principal series has been observed for them. The following table gives a summary of all the measurements for the second subordinate series.

SECOND SUBORDINATE SERIES.

Cu λ 4531	*Ag*¹ λ 4669	*Al* λ 3962	*Al* λ 3962	*Al*¹ λ 3962	*Al*² λ 3962	*Tl* λ 5351	*Tl*¹ λ 5351	*Tl*² λ 5351	Mean
−2.20	.....	−2.34	−2.31	−2.33	.....	−2.33	−2.25	.....	−2.293*s*
−1.37	−1.38	−1.39	−1.39	−1.39	.....	−1.40	−1.39	.....	−1.387*s*
−0.45	.....	−0.45	−0.53	.....	−0.47	−0.48	.....	−0.47	−0.475*p*
+0.39	.....	+0.50	+0.51	.....	+0.47	+0.48	.....	+0.47	+0.470*p*
+1.33	+1.38	+1.38	+1.40	+1.41	.....	+1.39	+1.36	.....	+1.379*s*
+2.30	.....	+2.30	+2.31	+2.31	.....	+2.35	+2.27	.....	+2.307*s*

Cu λ 4481	*Ag* λ 4476	*Al* λ 3944	*Al* λ 3944	*Al*¹ λ 3944	*Al*² λ 3944	*Tl* λ 3776	*Tl*² λ 3776		
−1.93	−1.78	−1.86	−1.86	−1.84	.....	−1.95	.....		−1.870*s*
−0.94	−0.93	−0.92	−0.89	.....	−0.93	−0.87	−0.85		−0.904*p*
+0.98	+0.92	+0.94	+0.88	.....	+0.93	+0.89	+0.86		+0.914*p*
+1.89	+1.80	+1.84	+1.87	+1.84	.....	+1.93	... ..		+1.862*s*

For the measurements designated by¹ the vibrations parallel to the lines of force, for those designated by² the vibrations perpendicular to the lines of force, were cut out by a calc-spar. It was not possible to measure all of the components of the silver line at λ 4669. We were able to perceive two components, but only the two stronger ones, only when the vibrations parallel to the lines of force were cut out. Equal weights were given to all the measures in forming the means. It would not be correct to give a lower weight to the aluminium line than to the lines of

copper and silver, for although the wave-length is less, the measurement is nevertheless equally accurate, since the line is stronger and the components are consequently more readily seen. The mean would be only slightly changed, if different weights were given to the measures. The mean error of a single measure is computed from the deviation from the mean to be 0.039, which can doubtless be attributed to the uncertainty of the observations. The means therefore have a mean error of 0.016 or 0.015.

A comparison of the means with the values computed for the principal series gives convincing evidence that within the limits of accuracy of the observations, the larger wave-length in case of the pairs of the subordinate series is separated in just the same way as the smaller wave-length in the pairs of the principal series, and *vice versa*. The means are collected in the following table:

SEPARATION OF THE PRINCIPAL SERIES, AND OF THE SECOND SUBORDINATE SERIES.

Second Subordinate Series. Greater Wave-Length	Principal Series. Smaller Wave-Length	Differences
−2.29 *s*	−2.25 *s*	−0.04
−1.39 *s*	−1.40 *s*	+0.01
−0.48 *p*	−0.45 *p*	−0.03
+0.47 *p*	+0.45 *p*	+0.02
+1.38 *s*	+1.35 *s*	+0.03
+2.31 *s*	+2.30 *s*	+0.01
Smaller Wave-length	Greater Wave-length	
−1.87 *s*	−1.86 *s*	−0.01
−0.90 *p*	−0.93 *p*	+0.03
+0.91 *p*	+0.94 *p*	−0.03
+1.86 *s*	+1.85 *s*	+0.01

The difference has a mean error of between 0.024 and 0.026, according to the mean error of the means as above computed. The square root of the arithmetical mean of the squares of the differences is equal to 0.025. This agreement shows that as far as we are permitted by the accuracy of the measurements to draw conclusions, the separation is the same for the principal series and for the second subordinate series.

As to the first subordinate series, the two principal lines are split up into three lines each by the magnetic field. But the satellite, which accompanies the principal line of smaller vibration number on the side toward smaller vibration numbers, consists in the magnetic field of eight components, of which six vibrate perpendicularly and two parallel to the lines of force. The measurements for the four elements are collected in the following table:

FIRST SUBORDINATE SERIES.

Cu λ 5220.2	*Ag* λ 5471.7	*Ag* λ 5471.7	*Tl*¹ λ3529.6	*Tl* λ 3529.6	*Tl*² λ 3529.6				Mean
−2.26	.....	.....	−2.09 *s*	−2.20	.....				−2.18 *s*
−1.07	−1.15	−1.12	−1.40 *s* −0.84 *s*	−1.11	−1.07 *p*				−1.10 *p*
+1.12	+1.15	+1.12	+0.83 *s* +1.49 *s*	+1.11	+1.07 *p*				+1.11 *p*
+2.20	+2.22	.....	+2.01 *s*	+2.20	.....				+2.16 *s*
Cu λ 5218.3	*Ag* λ 5465.7	*Ag* λ 5465.7	*Ag* λ 5465.7	*Al*¹ λ 3092.8	*Al* λ 3092.8	*Tl*¹ λ 3519	*Tl* λ 3519	*Cu*¹ λ 4062.9	
−1.52	−1.52	−1.48	−1.54	−1.42	−1.39	−1.52	−1.50	−1.40	−1.48 *s*
+0.02	0.00	0.00	+0.03	.....	−0.03	.....	0.00	.....	0.00 *p*
+1.50	+1.52	+1.48	+1.51	+1.42	+1.39	+1.52	−1.50	+1.40	+1.47 *s*
Cu λ 5153.3	*Ag* λ 5209.2	*Ag* λ 5209.2	*Al* λ 3082.3	*Al*¹ λ 3082.3	*Tl*¹ λ 2768	*Cu*¹ λ 4022.8	*Cu* λ 4022.8		
−1.12	−1.19	−1.14	−1.21	−1.26	−0.96	−1.20	−1.10		−1.15 *s*
−0.04	+0.02	−0.01	−0.03	0.00	.....	.....	−0.03		−0.01 *p*
+1.16	+1.18	+1.15	+1.24	+1.26	+0.96	+1.20	+1.12		+1.16 *s*

In the case of the plates designated with[1] the vibrations parallel to the lines of force, in the case of those designated with[2] the vibrations perpendicular to the lines of force, are cut out. Only in the case of thallium did we succeed in completely separating the satellites. In the case of the other elements the components vibrating parallel to the lines of force run together with those adjacent vibrating perpendicularly, so that we can only observe the center, which coincides with the components vibrating parallel to the lines of forces. The separation of the satellite could not be observed at all in the case of aluminium. Two other faint components could be observed near the aluminium lines at

λ3082.3, presumably belonging to another line lying near to λ3082.3. Equal weights are given to all measures in taking the means. The means are computed in case of the satellite only for the two outside components vibrating perpendicularly to the lines of force, and for the two components vibrating parallel to the lines of force.

The mean error of a single measure from the deviations from the mean is computed to be 0.065, which is explained by observation errors, and would not justify any conclusion that the separation of corresponding lines is a little different from the different elements. The greatest deviations from the mean occur in the case of the thallium line at λ2768, and correspond here to a difference of wave-length of about 0.015 tenth-meters. The components lie very close together on account of the short wave-lengths and are very difficult to measure separately.

Kayser and Runge have observed in the spectrum of copper still another pair of lines, with wave-lengths λ5782.30 and λ5700.39, which does not belong to the series, although the vibration numbers have the same difference as in case of the pairs. It is interesting to note that the separation of the two lines by the magnetic field conforms to the separations occurring in the first subordinate series. The smaller vibration number has the separation of the larger vibration number in the pairs of the first subordinate series, and the larger vibration number has conversely the separation of the smaller vibration number in the pairs of the first subordinate series, namely, the separation of the satellites. The agreement is shown in the following table.

The reversal in the succession for the lines of the two pairs recalls the relationship of the second subordinate series to the principal series. We should be led to the suspicion that there are two principal series, of which one bears a relation to the first, the other to the second subordinate series, but for the present this must remain only as a suspicion.

Kayser and Runge also observed in the spectrum of silver a pair of lines not belonging to the series and corresponding to the pair in the spectrum of copper just mentioned. We have unfor-

Cu λ5782.3 Greater Wave-length of Pair.	First Subordinate Series Smaller Wave-length Mean.	Remarks
− 1.15	− 1.15	
0.00	− 0.01	
+ 1.15	+ 1.16	

Cu λ5700.4 Smaller Wave-length of Pair.		First Subordinate Series. Greatest Wave-length. Satellite. *Tl.*		Remarks
a	*b.*	*a.*	*b.*	In the measures under *b*, the vibrations parallel to the lines of force were cut out by a calc-spar. In the measures under *a*, the two vibrations parallel to the lines of force so fall upon those vibrating perpendicularly that the neighboring perpendicular vibrations cannot be separated from them.
− 2.20	− 2.32	− 2.20	− 2.09	
− 1.18	− 1.52	− 1.11	− 1.40	
	− 0.68		− 0.84	
+ 1.12	+ 0.72	+ 1.11	+ 0.83	
	+ 1.51		+ 1.49	
+ 2.26	+ 2.30	+ 2.20	+ 2.01	

tunately not succeeded in observing this pair in the magnetic field, as it cannot be seen on our plates.

All these types observed in the series of pairs in the spectrum of *Na*, *Cu*, *Ag*, *Al*, and *Tl* also occur in a number of pairs found in the spectra of *Mg*, *Ca*, *Sr* and *Ba*, and from this we should be inclined to regard them also as series lines, although they cannot be resolved into series, but always give only a single representative of a series. In respect to the number of their components as well as to their distances and their ratios of intensity, the separation is the same in all details. We should, therefore, not hesitate to apply to these lines the same designation as principal series, first and second subordinate series. Some of the strongest lines in these spectra are included among these lines; for instance, the calcium lines H and K without doubt constitute a pair of a principal series, and therefore correspond to the two D lines in the spectrum of sodium. The following table contains the measurements, together with the means of the distances of the corresponding lines of *Na*, *Cu*, *Ag*, *Al*, and *Tl*.

The vibrations parallel to the lines of force were cut out in case of the measurements designated by [1], those perpendicular to the lines of force where designated by [2]. The principal series and the second subordinate series are difficult to observe in case of

Principal Series Greater Wave-length				Second Subdivision Series Smaller Wave-length				Mean		Mean of Corresponding lines in Na, Cu, Ag, Al, Tl	
Mg λ2803	*Ca* λ3969	*Sr* λ4215.7	*Ba* λ1934	*Ma* λ2939	*Ca* λ3706	*Sr* λ4162	*Ba* λ4525	Principal.	Subordinate.	Principal	Subordinate
−1.75	−1.82	−1.84	−1.83	−1.79	−1.83	−1.85	−1.80	−1.81*s*	−1.82*s*	−1.86*s*	−1.87*s*
−0.98	−0.91	−0.98	−0.94	−0.99	−0.89	−0.93	−0.96	−0.95*p*	−0.94*p*	−0.93*p*	−0.90*p*
+0.92	+0.94	+0.97	+0.93	+0.99	+0.89	+0.93	+0.96	+0.94*p*	+0.94*p*	+0.94*p*	+0.91*p*
+1.79	+1.80	+1.84	+1.84	+1.79	+1.83	+1.84	+1.84	+1.82*s*	+1.82*s*	+1.85*s*	+1.86*s*
Principal Series Smaller Wave-length				**Second Subordinate Series Greater Wave-length**							
Mg λ2796	*Ca* λ3933.8	*Sr* λ4078	*Ba* λ4554	*Mg* λ2037	*Ca* λ3737	*Sr* λ4306	*Ba* λ4900				
−2.22	−2.25	−2.32	−2.26	.....	−2.27	−2.27	−2.22	−2.26*s*	−2.25*s*	−2.25*s*	−2.29*s*
−1.37	−1.36	−1.39	−1.37	−1.44	−1.41	−1.37	−1.38	−1.37*s*	−1.40*s*	−1.40*s*	−1.39*s*
−0.41	−0.45	−0.49	−0.48	−0.33	−0.43	−0.47	−0.46	−0.46*p*	−0.42*p*	−0.45*p*	−0.48*p*
+0.41	+0.48	+0.45	+0.46	+0.33	+0.43	+0.45	+0.45	+0.42*p*	+0.42*p*	+0.45*p*	+0.47*p*
+1.37	+1.33	+1.38	+1.36	+1.44	+1.37	+1.37	+1.33	+1.36*s*	+1.38*s*	+1.35*s*	+1.38*s*
+2.22	+2.27	+2.36	+2.28	.....	+2.32	+2.28	+2.27	+2.28*s*	+2.29*s*	+2.30*s*	+2 31*s*

FIRST SUBORDINATE SERIES.

	*Ca*¹ λ3181.4	*Ca*² λ3181.4	*Sr*¹ λ3475	*Sr*² λ3475	*Sr* λ3475	*Ba*¹ λ4166.2	*Ba*² λ4166.2	Mean of the corresponding lines of *Cu*, *Ag*, *Al*, *Tl*
Satellite	−1.45		−1.41		−2.28	−2.06		−2.18*s*
						−1.46		−1.40*s*
		−1.09		−1.09	−1.14		−1.11	−1.10*p*
						−0.78		−0.84*s*
		+1.09				+0.70		+0.83*s*
				+1.09	+1.10		+1.11	+1.11*p*
	+1.45		+1.41			+1.44		+1.49*s*
					+2.32	+2.16		+2.16*s*
	Mg λ2798	*Ca* λ3179.4	*Sr* λ3465	*Ba* λ4130.9				
	−1.38	−1.52	−1.53	−1.49				−1.48*s*
	0.00	0.00	0.00	−0.02				0.00*p*
	+1.38	+1 52	+1.53	+1.51				+1.47*s*
	Mg λ2791	*Ca* λ3159 0	*Sr* λ3381	*Ba* λ3892.0				
	−1 04	−1 12	−1.17	−1.12				−1.15*s*
	0.00	0.00	+0.03	+0.01				−0.01*p*
	+1.04	+1.12	+1.14	+1.11				+1.16*s*

Mg, since these lines reverse easily. The appearance of the line on the photographic plate may thus turn out very differently, according as the line was reversed during the whole or during a part of the exposure. We were only able to measure those plates on which no reversal was to be seen. In addition the wave-lengths of the line are small and consequently the differences of wave-lengths of the components are slight. The

separation of the satellite is also difficult to observe in the first subordinate series, and we only succeeded in obtaining the complete separation in the case of barium. Nevertheless, it seems to us as not open to doubt that the separation of the satellite is the same for the other elements. On cutting off the vibrations parallel to the lines of force, the components vibrating perpendicularly to the lines of force run together into two diffuse lines. If none of the components are cut out, the stronger components vibrating parallel to the lines of force are superposed upon the others. We cannot then distinguish from them the neighboring components vibrating perpendicularly, and we also perceive in addition to them the outside components vibrating perpendicularly. No satellite was observed in the case of *Mg*, either because none exists, or, as is more likely, because it falls so near to the principal line that it is concealed by it.

In the spectrum of barium, as of copper, still another pair was found, yielding the same difference in the vibration numbers, although not belonging to the three series. The wave-lengths are λ 6497.07 and λ 5853.91. We measured the separation in the magnetic field of the latter and found that it agreed with the separation of the corresponding copper lines, and had the type of satellite of the first subordinate series. The distances of the components are given in the following table :

Ba [1] λ 5853.9	*Ba* [2] λ 5853.9	*Cu* λ 5700.4	Remarks
— 2.19*s*		— 2.26*s*	[1] Vibrations parallel to the lines of force are cut out.
— 1.47*s*		— 1.52*s*	
	— 1.14*p*	— 1.18*p*	[2] Vibrations perpendicular to the lines of force are cut out.
— 0.73*s*		— 0.68*s*	
+ 0.72*s*		+ 0.72*s*	*s* Vibrations perpendicular to the lines of force.
	+ 1.14*p*	+ 1.12*p*	
+ 1.47*s*		+ 1.51*s*	*p* Vibrations parallel to the lines of force.
+ 2.19*s*		+ 2.28*s*	

We did not measure the other line at λ 6497.007, but we were able at least to satisfy ourselves that it is resolved into a triplet by the magnetic field, like the corresponding copper line.

The principal results of this paper are these:

1. The pairs of lines observed in the spectra of the elements *Na*, *Cu*, *Ag*, *Al*, *Tl*, *Mg*, *Ca*, *Sr*, and *Ba*, when separated by the magnetic field, exhibit a number of types which repeat themselves from element to element. Here the separations of the same type agree to the smallest detail for the different elements; that is, not only is the number of components the same, but also their ratios of intensity, and their distances when we consider the lines as represented on a scale of vibration numbers.

2. The pairs of lines may be arranged according to types, as those of the principal series, those of the first subordinate series and those of the second subordinate series. The types of the principal series and of the second subordinate series are the same, but with the succession inverted, as was to be suspected *a priori*, according to the relation found by Rydberg between the principal and the second subordinate series.

3. The distances of the components of the principal series and of the second subordinate series from the unaffected line are multiples of the same number on the scale of vibration numbers. They are equal to the even multiples for the one line and to the odd multiples for the other line. We have previously communicated a similar law in case of the second subordinate series of triplets in the spectrum of *Mg*, *Ca*, *Sr*, *Zn*, *Cd* and *Hg*. The number which in that case represented by its multiples the distances of the component stands very nearly in the ratio of 3:2 to the number found here.

The experimental part of this investigation was carried on by the authors in common, but on account of his removal to Tübingen, F. Paschen was unable to participate in the measurement of the photographic plates or in the discussion of the measures.

OBSERVATIONS OF THE AURORA MADE AT THE YERKES OBSERVATORY, 1897–1902.

By E. E. Barnard.

Williams Bay seems to be well situated for observations of the aurora. When we first came here in 1896–7, there were frequent and brilliant auroras. In the succeeding years they became less so, and with the exception of what seemed to be special outbursts they appeared to die out altogether.

A few of the first were not recorded, but when the record began a strict account was kept of all auroras seen, and frequently their absence was noted at times near when they had been seen, or for other reasons.

Some of the phenomena of these displays were wholly new to me. My previous experiences had been with very inconspicuous auroras, with the exception of the magnificent display of April 16, 1882, which I saw at Nashville, Tenn., and which I have not seen equaled since.

Following is a list of the records made here, which may be important, as they cover a Sun-spot minimum, and will doubtless bear on the connection between Sun-spots and auroras.

Ninetieth meridian time is used in these notes.

1897.

March 27. $8^h\ 10^m$. A faint aurora has appeared 10° east of the north point. It is decided but not bright, and stretches along some 30° of the horizon.

April 23. Fine aurora from 8^h. Finest at 9^h, when extraordinarily sharp arch formed. Fine streamers.

July 30. Fine aurora in early part of night, but no streamers had appeared when it clouded up. Cleared at $14^h\ 30^m$. From this time on till daylight the aurora was very strong, with a well-formed arch, very dark beneath—streamers not mentioned and probably not present.

August 19. Magnificent aurora all night. Very active at $12^h\ 30^m$ to $13^h\ 0^m$.

August 29. Auroral arch at midnight; died out before dawn. Did not notice any streamers.

October 29. Fine aurora from 9^h to 10^h.

December 20. $9^h\ 15^m$. Through breaks in the clouds in the low north there is a brilliant aurora with streamers moving to the *left*. It is evidently a very bright affair; cleared at $9^h\ 30^m$; the aurora kept up until about 10^h, with brilliant streamers, then gradually died down. At $13^h\ 5^m$ there were no streamers, but a diffused auroral glow with rapid pulsations — horizontal strips of flashing light.

$15^h\ 50^m$. The aurora seems to have died out.

$16^h\ 0^m$. The aurora is not dead. The same strange rapid flares of light are going on.

$16^h\ 30^m$. The aurora is still fluttering. At a small altitude a bright strip of horizontal light will appear and rapidly spread to the *left*, and then back again and out. The motion is extremely rapid.

December 21. $14^h\ 10^m$. I see the glow and a smoky arch for the first time tonight. It did not make any display up till daylight.

December 22. Up to $7^h\ 30^m$ no aurora had appeared.

December 23. No aurora as late as 10^h.

December 24. No aurora — clear till $16^h\ 30^m$.

1898.

January 15. Cleared at 12^h; from midnight until $14^h\ 30^m$, when it clouded again, a strong auroral glow.

January 16. At $9^h\ 30^m$ there was a bright aurora with streamers which moved slowly to the *left*. Heavy arch. This died out in half an hour. As late as $14^h\ 30^m$ it had not again appeared.

Observer absent from January 29 to February 16.

March 14. $7^h\ 20^m$. There is a fine long auroral arch, no streamers. A vertical line from β *Ursae Minoris* will cut the exact summit of the arch. From this on until $8^h\ 30^m$ the aurora was superb — a great double arch and magnificent streamers of a crimson color — the aurora filled all the north up to and above the pole. At about $8^h\ 10^m$ there were persistent white masses in the northwest, low near the horizon. At $8^h\ 40^m$ the aurora is quieter, with large white glow from north horizon and whitish masses in the low northwest. This is by far the finest aurora I have seen here. $10^h\ 0^m$. Fine aurora again. The arch remained as late as 11^h, when it clouded up, though the activity had ceased. At 10^h *Vega* could be seen shining brilliantly through the dark space below the brilliant arch.

March 15. $7^h\ 10^m$. Sky covered with a magnificent aurora clear to the zenith; at $7^h\ 15^m$ great streamers running south of zenith to the southeast. Rapid pulsations in the light in the north all the time. The streamers below pole moved to the *left*. The pulsations seemed to be waves of light concentric with the arch, ascending with great velocity and following each

other rapidly at intervals of one or two seconds. Twice a brilliant and enormously long irregular ray of light about 1° or 2° broad stretched across the sky south of the zenith and perpendicular to the meridian. This had a slow motion to the south and was sinuous. A white, comet-like ray — perfectly resembling a large comet — extending from near the east horizon through *Jupiter*, remained stationary for upwards of an hour. Patches and wisps of nebulous light appeared in all parts of the sky.

$9^h\ 50^m$. The sky has been luminous all over the north for some time, though quiet, but the arch is forming again. It is a very long, low arch whose center is in the same vertical with *β Ursae Minoris*, and whose altitude is one-fourth of that of the star.

$12^h\ 0^m$. The aurora has been active for the past half hour — splendid streamers shooting up from the arch. These streamers all move very slowly to the left. In the beginning, before the arch broke, bluish white masses of intense light appeared on the arch and moved very rapidly to the *right*.

$12^h\ 50^m$. The aurora is still very active, sending up streamers as high as the pole.

$14^h\ 0^m$. The aurora is dead except for occasional flashings up.

$15^h\ 0^m$. Very quiet but occasional flashings up.

March 16. No trace of aurora as late as $11^h\ 30^m$.

March 19. $10^h\ 35^m$. There is an auroral glow in the north which has appeared within the hour. $10^h\ 55^m$. The aurora is getting brighter, with arch and faint streamers. $12^h\ 10^m$. The aurora is moderately bright and slightly active. The streamers are ordinary and move to the *left*.

The aurora died down after midnight, the arch and glow disappearing. Two brilliant fluctuating clouds, however, remained, or rather appeared, after the arch had gone; one of these was almost due east and the other was west of the pole. These would brighten up for a few seconds very brilliantly and then die down again. The one to the west remained thus until early dawn.

March 20. From $15^h\ 30^m$ to $17^h\ 20^m$ no aurora.

March 23. No aurora during the night.

March 24. No aurora until midnight, when streamers shot up for half an hour and then died out.

$13^h\ 30^m$. The aurora seems to have entirely died out.

March 28. $14^h\ 10^m$. An auroral glow under pole has started up. No other remarks, though observing all night until 18^h.

March 29. $14^h\ 0^m$. There is a slight aurora.

April 20. No aurora during the night. Clear all night.

May 3. $13^h\ 0^m$. There is a strong auroral arch in the north which, in spite of Moon and haze, is conspicuous and shoots up streamers. This is the first aurora since the record of March 29. I have kept a close lookout for the aurora on every clear night.

Observer absent from May 24 to June 8.

August 12. Aurora began faintly at $8^h\ 30^m$. Brightest at $10^h\ 30^m$, when there was a strong arch and feeble effort at streamers. After this it died out, and at $13^h\ 30^m$ there was only a feeble glow.

September 2. $10^h\ 15^m$. There is a strong double (concentric) aurora arch. At $10^h\ 25^m$ the aurora is extremely brilliant and active, though the Moon is nearly full. It makes a brilliant display of streamers and breaks up into great masses of intense light. The display is unusually fine.

$11^h\ 50^m$. The aurora has been dead for an hour or so. No further record of it as late as $16^h\ 15^m$.

September 10. $9^h\ 0^m$. There is a very faint auroral glow in the north, $11^h\ 30^m$. There is a strong aurora with arch. $12^h\ 30^m$. The aurora is active, it (the arch) breaks up into masses of light but no streamers.

$13^h\ 0^m$. A magnificent and superb display of aurora.

The most striking feature of this was a great comet-like mass of intense light with head to the southwest of *Orion*, and stretching across the sky slightly south of the zenith, to the west horizon. It was some 20° wide and very much resembled some of the photographs of Brooks' comet of 1893. The stars shone through the brightest part of it. It moved slowly to the southeast, and faded out about $13^h\ 10^m$, but would then brighten up near *Orion*. So bright was the display on this date that at times the light in the north cast a distinct shadow of a person across the ground.

September 11. No aurora at night.

September 12. $8^h\ 10^m$. There is an intense auroral light shining through the clouds in the northwest in the region of *α Canum*. It is very brilliant and makes a glow through the rather thin clouds. It seemed to be a large roundish mass of light.

September 15. 14^h. There is an auroral spot 10° above the horizon in the northwest and a similar one in the northeast 10° high. These faded and brightened rapidly. By $16^h\ 30^m$ the one in the northwest had extended in a fragment of arch nearly under the pole, and was very bright as daylight killed it out. The one in the northeast had disappeared.

September 19. Dense smoky yellow haze all day. Could not see across the lake (1½ miles). Though the night was more or less broken with clouds, observed all night, and there is no record of aurora. This haze doubtless had nothing of an auroral nature.

October 14. Auroral glow from 11^h until 12^h; no arch. Feeble streamers for a while.

November 5. No aurora.

November 7. No aurora.

November 10. No aurora.

November 11. $7^h\ 30^m$. During the past ten minutes a feeble aurora has started up in the north.

$9^h\ 35^m$. The aurora is quite strong, with feeble efforts at streamers.

$10^h\ 15^m$. The aurora is brighter.

$12^h\ 40^m$. The aurora has dimmed down.

November 22. $15^h\ 0^m$. Faint aurora, which did not last long.

November 23. No aurora.

December 13. $9^h\ 50^m$. An aurora with a low arch has started in the north. It is moderately bright. There has been no aurora for a long time. I have looked out for auroras every clear night. $13^h\ 23^m$. The aurora is very strong — like a strong dawn, but very low. It extends pretty far east. There is no definite arch or streamers. $15^h\ 30^m$. The auroral glow is still strong among the clouds.

1899.

January 28. $10^h\ 0^m$. Aurora before Moon-rise and strong arch after Moon-rise.

February 11. $8^h\ 0^m$. Strong auroral arch — the first seen since January 28. $11^h\ 50^m$. The arch is very bright, but there are no streamers. $12^h\ 5^m$. The auroral arch is now breaking up. *Vega* is shining through the darkest part under the arch. Temperature 23° (F.) below zero. $14^h\ 50^m$. The aurora is very active. It is splendid. There are no streamers above the arch, but there are brilliant masses of bluish-green light, like cometary tails, projecting up in the arch and moving to the right. There are quick waves of ascending light, and the arch is broken, and double in places. This is the finest display I have seen in a long time. $15^h\ 20^m$. The auroral arch is broken up into cloud masses — no definite arch, but dark below. The ascending waves of light are very rapid all along the line. Temperature, 24° (F.) below zero. $17^h\ 30^m$. The aurora is very active. Great streamers. No pulsating waves. $17^h\ 15^m$. The aurora is bright and sending up streamers to the east. Temperature, 25°.5 (F.) below zero.

February 12. No aurora during the night.

February 15. $12^h\ 5^m$. There is a fragment of a bright arch in the northeast over *Vega*. It is about one-fourth of an entire arch, beginning at the northeast horizon and ending abruptly. There is no glow or indication of auroral light anywhere under the pole or near it. *Vega* shines through the dark part under the fragment of arch.

May 1. $8^h\ 35^m$. There is a pretty strong auroral arch. $9^h\ 25^m$. The arch is quite strong — no streamers. $14^h\ 30^m$. The aurora has nearly died out. It was very active about 11^h. The arch was low, $= \frac{1}{10}$ altitude of *Polaris*. There was a thin inner arch part of the time, and some bright spots forming and a few streamers.

May 2. No aurora during the night.

May 4. $11^h\ 0^m$. There is a rather noticeable aurora that started up about 10^h. By midnight the aurora was very bright.

May 18. A faint aurora was visible from 13^h to 14^h.

June 28. Cleared at midnight, showing a strong and active aurora. This was bright in spite of the Moon at $13^h\ 0^m$, but died out shortly after $14^h\ 0^m$.

June 29. $9^h\ 0^m$. The sky is partly covered with thin hazy clouds. There is the eastern part of a decided auroral arch seen under the clouds. To the left of the pole the arch cannot be seen on account of twilight and clouds. There is a brightish streamer in the east near the horizon, inclined 10° or 15° to the south at upper end. $9^h\ 30^m$. The full arch is very strong. It seems to me that the center of the arch is further to the right than usual.

$10^h\ 5^m$. The aurora is brighter. There are a few faint streamers to the left of the pole. The arch is not black beneath — it is a light gray; there is little contrast with the bright arch. The sky is darker above the arch, but is covered with a diffused glow. The aurora became very active about 11^h and threw up streamers. By $11^h\ 30^m$ it seemed to have nearly died away. There were bright greenish-blue-white masses in the lower part of the arch that moved rapidly to the *right* as they faded.

June 30. Up to $13^h\ 30^m$ there had been no aurora, but at $14^h\ 15^m$ a feeble aurora started up. $15^h\ 30^m$. The aurora did not get fairly started before dawn killed it out.

July 1. No aurora during the night.

July 2. Clouded at 11^h. Before this there was no aurora.

July 5. $10^h\ 10^m$. There is a distinct but feeble auroral spot 20° to the left of north at 10° or 15° altitude, but no other indications of aurora. $10^h\ 40^m$. The luminous appearance now seems to be almost under the pole and to slant to the east at its upper end.

July 8. No aurora during the night.

August 5. $9^h\ 10^m$. There is a patch of auroral light to the right of the pole at an altitude of some 10° and several faint streamers at that point. There has been no aurora during any of the nights of observation lately. A close watch has been kept.

September 12. $12^h\ 0^m$. There is a slight aurora very low on the north-northeast horizon. $14^h\ 35^m$. The auroral light has all gone. There was no display.

September 15. $15^h\ 35^m$. For the past half hour a strong auroral glow has been visible low in the north. It has now become quite bright.

September 25. $10^h\ 0^m$. Strong auroral glow — the first in a long time.

September 26. $7^h\ 40^m$. Slight auroral glow in the north. $10^h\ 50^m$. The aurora has been very feeble, but it is now strong, with a heavy arch. $12^h\ 0^m$. There are some streamers in the north.

September 30, October 1, October 2. No aurora during the night.

October 9. No aurora during the night.

October 12. No aurora during the night.

October 28. No aurora.

November 3. $13^h\ 0^m$. There was a strong auroral glow in the north horizon, but it did not last long.

November 4. No aurora.

Observer absent from December 8 to 1900, March 8.

1900.

April 30. $9^h\ 40^m$. Strong aurora in north, with streamers one-half way to pole. Arch low and irregular. $10^h\ 40^m$. The aurora seems to have died out, except a faint glow.

May 4. $12^h\ 0^m$. There is an auroral glow, with dark clouds below it in the north. $13^h\ 30^m$. There is a dark bank of clouds in the north with the aurora. The auroral light is pretty strong, but irregular. No streamers.

Observer absent from May 8 to June 1.

September 27. $14^h\ 5^m$. There is a slight auroral glow under the clouds in the north. No arch.

1901.

Observer absent from February 6 to July 26.

November 18. $13^h\ 30^m$. A slight auroral display. A few streamers shot up from the north horizon under the pole.

1902.

April 10. Cleared at midnight, exposing a rather bright auroral glow in the north horizon. This increased and finally formed a distinct arch — dark below ; the lower part, or base of the arch, being only 5° or 6° high. The top of the brightness just reached to α *Cassiopeiae*. It was most distinct at $13^h\ 0^m$. By $14^h\ 0^m$ it had quite faded out. There were no streamers while I watched it. The arch was quite bright, and was very low and long. This is the first aurora I have seen since the last record some months ago (November 18).

May 8. $13^h\ 0^m$. There is a very low auroral arch in the north. It is dark beneath and was not there before midnight. $14^h\ 30^m$. The aurora is very strong. The space beneath it is very dark. The arch is bent down in the middle as if it were made of two arches touching each other. There are faint streamers to the right of north. $14^h\ 45^m$. It is intensely bright, casting a shadow. There are outrushes of bright matter from the dark part, to the left of north, that form very faint, but not decided streamers. These bases of streamers, as it were, eat into the dark arch and break its symmetry badly. $15^h\ 10^m$. It has all died out, and has left only a faint glow. The aurora seemed to be most active to the left of the pole. The top of the dark arch — base of bright arch — was only 5° high — the top of bright arch about 10° high.

In connection with the magnificent aurora visible here on September 10, 1898, it is well to call attention to the fact that this

disturbance seems to have been general, as great displays were visible in England and on the continent about this time, mainly on September 9.

In speaking of the display of September 9, in Finland, Baron Kaulbars says the aurora was "not only one of the most splendid seen, but also that has appeared in our latitude for a long series of years." It ceased about midnight in Finland.

In *Nature* for 1898, September 8, Mr. C. E. Stromeyer, of Whitby, England, says that on September 2, from $7^h\ 45^m$ to $8^h\ 15^m$ there was a display visible with the center of rays apparently resting on the horizon, about north 25° east. "The rays revolved from west to east at the rate of about 20° in 10^m."

In an interesting account of the aurora of September 9, given in *Nature* for September 15, 1898, pp. 490–491, it is stated that there were bright auroras visible in England also on September 2, 7, and 10.

> In connection with the recent display it is interesting to note that the unusually large spot which came over the eastern limb of the Sun on Saturday, September 3, was on the central meridian of the Sun's disk on Friday, the 9th, at about the time the aurora was at its maximum.
>
> And still further, the automatic recording instrument for magnetic declination in the physics department at South Kensington showed a large disturbance the same evening. From the photographic record it appears that the disturbance began about 7:30 P. M., and in fifteen minutes reached a value of 30′ of arc; by 8:00 P. M. the declination was normal again, but immediately afterward the needle traveled on in the opposite direction to the first displacement, and reached a second maximum eastward about 8:15. By 8:30 the needle had again assumed its normal position, and no further disturbance, other than the usual diurnal one, has yet been recorded. Thus the declination magnet was deflected over 1° in the hour from 7:30 to 8:30 P. M. This leaves little doubt as to the definite connection between the position of the spot on the solar disk, the magnetic variation, and the aurora.

A letter from Dr. Charles Chree in the same number of *Nature*, p. 468, shows that a conspicuous magnetic storm was recorded at the Kew Observatory while the aurora was in progress. In ending his letter Dr. Chree says:

> The curves had become fairly quiet by midnight of the 9th, but there was a recrudescence of the disturbance between 8:00 A. M. and midnight of the 10th, and subsequent smaller movements occurred on the 11th.

There are several features in connection with these auroral displays that will bear special notice.

When the arch is strongly formed, the space beneath it to the horizon has usually a dark, smoky look as if there were a bank of dark, smoky clouds filling all beneath the arch. This gives the impression of being densely opaque. But it is really transparent, as shown by several observations of Vega through its apparently densest portion. It would appear, therefore, that this smoky blackness is merely due to contrast with the bright arch above. This is very surprising, for if there is any material substance that fills this space it is certainly transparent. The motion of the streamers is moderate. They move slowly, say a degree in a couple of minutes, towards the left. (I use the terms right and left to avoid ambiguity.) Mr. Stromeyer in England (*Nature*, September 8, 1898) saw them moving from west to east, *i. e.*, in the reverse direction to the motion they have here. It seems to me that this is an important feature.

The intensely bright bluish or greenish-white balls that appear in the base of the arch have a very rapid motion from the left to the right, contrary to the motion of the streamers, and the velocity is very much greater than that of the streamers. They burst out very bright and move rapidly to the right along the arch and fade out as they go, usually existing for only a portion of a minute.

The pulsating clouds of light are remarkable features. They are usually 5° or 10° in diameter and very bright—for a short interval they will almost fade out and then quickly become very bright again—as if some one were capriciously turning on and off their light.

The comet-like objects are as nearly like a great naked-eye comet as possible. Their light is of the same color as that of a comet and their form almost that of a comet with a straight tail. The one seen on March 15, 1898, in the east near Jupiter, could easily have been mistaken for a real comet.

The arches are very various in their heights. Sometimes the arch is very low, being only a few degrees above the north

horizon. At other times it reaches nearly half-way to the pole. The center of the arch is usually 20° or 25° east of north.

It seems to me that a faithful record of the altitude of the arch and the azimuth of its summit must be important. Just why the arch should at one time be close to the horizon and at another time many degrees higher, will doubtless be an important feature when the nature of the aurora is better known. To stop and locate these features with reference to the stars when one is busy with other work, and especially to reduce such observations, requires considerable time from one who is busy enough with other work in which he is more especially interested. I have therefore thought that a crude instrument made of wood, consisting of a horizontal and a vertical circle roughly graduated to degrees, with a wooden rod as pointer, might be constructed and used for quickly observing the altitude and azimuth of the arch. Such an instrument we hope soon to have placed on the roof of this Observatory for quickly locating important features of the aurora.

The incomplete arch of February 15, 1899, was a singular feature and unique so far as my observations go.

The appearance of an aurora here is a capricious affair; there seems to be no means of telling when one will occur.

A very active aurora will appear one night, and the next night there will be no trace of anything of the kind, nor perhaps, had there been any trace of one the night before.

The bright auroras that we shall doubtless have in the next ten years will perhaps give important information from a spectroscopic standpoint. Some of the principal features should also be easily photographed with short-focus, quick-acting lenses, but the exposures must be very short, because of the motion of the streamers.

These observations of aurora have been made in the hope that they might some day be valuable. It seemed a pity that no record should be kept of their appearance, though my interest has lain in other directions altogether.

YERKES OBSERVATORY,
September 2, 1902.

NOTES ON SPECTRO-PHOTOMETRIC ADJUSTMENTS.

By L. B. TUCKERMAN.

ABOUT two years ago[1] Professor D. B. Brace described a new and simple form of spectro-photometer. Several of these instruments have been since in almost continuous successful use in the laboratory of the University of Nebraska. In this time several alterations have been made in the design of the instrument which render its adjustment much simpler and easier. In view of the growing importance of spectro-photometric measurements in various physical and chemical-physical investigations it has been thought worth while to describe the modified instrument in connection with a discussion of the conditions which afford the greatest accuracy with ease of observation.

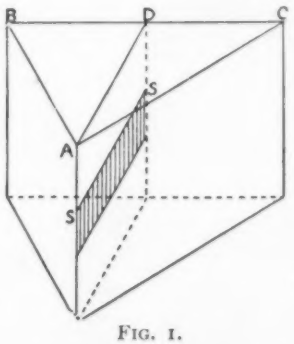


FIG. 1.

The instrument consists essentially of a double prism P (Figs. 1 and 2) with a narrow silvered strip SS on the face AD of the right half. Light of the same wavelength is thus brought by direct transmission from the collimator T, and after reflection at the silver strip from T', to the same focus in the observing telescope R. When the eyepiece is removed and the prism viewed through a slit in the focal plane of R, the eye sees three fields (Fig. 3), the central one $ABCD$ illuminated by light from the right collimator T', and the upper and lower ones, ACF and BDG, by light from the left collimator T. The fields meet in the sharp edge of the silver strip.

Lummer and Brodhun[2] have given the following three condi-

[1] *Phil. Mag.*, **48**, 420–430, 1899, and ASTROPHYSICAL JOURNAL, **11**, 6–24, 1900.

[2] *Zeitschrift für Instrumentenkunde*, **9**, 42, 1889.

tions which should be met by an ideal photometer screen where the principle of equality is used.

1. Each of the comparison fields must receive light from only one light-source;

2. The bounding line between the two fields must be as sharp as possible; and

3. It must vanish at equal illumination of the two fields.

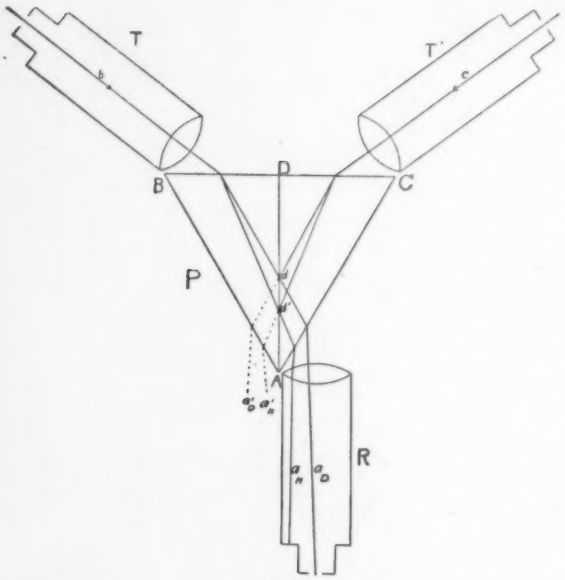


FIG. 2.

The third condition evidently includes uniform illumination of each field and for spectrophotometric work the added condition that the field be of uniform tint throughout. The first of these conditions is evidently reached if the cementing substance between the two half-prisms has a refractive index sufficiently near that of the glass. (For glasses of low refractive index (*circa* 1.5), balsam may be used, but for those of higher index *a*-monobromonaphthalene is more satisfactory.) The second condition is fulfilled even with non-homogeneous light when the edges of the silver strip are perpendicular to the refracting edge of the prism.[1] To secure a field of homogeneous tints when the slits are illuminated with white light, the light must pass in parallel rays through the prism, to retain its homocentricity, and the ocular slit must lie in the same plane as the image of the collimator slit and be somewhat narrower than the pupil of the

[1] O. LUMMER and E. BRODHUN, *Zeitschrift für Instrumentenkunde*, 12, 137–138 1892.

eye.[1] With these conditions fulfilled, the tint of the field appears uniform throughout, while with imperfect focusing the tint varies from one side of the field to the other.

In adjusting the instrument for use, more trouble was found in meeting the remaining requirements. The illumination would in general vary in intensity across the field, causing the appearance shown in Fig. 4. Because of the sensibility of the instrument a variation of one or two tenths of 1 per cent. across the field was noticeable and troublesome in careful work. This uneveness might be due: (1) To an uneven silver deposit. Care in the silvering, especially in insuring that the temperature of the silvering bath and the prism should not be widely different, easily obviated this. (2) To an imperfect slit. If one of the jaws of the slit were slightly displaced along the axis of the collimator (Fig. 5), the effective width of the slit was greater on one side of the field than on the other. This trouble should not be present in any good slit. (3) To unsymmetrical path of the two collimators. Owing to the more oblique reflection at the edges of the lenses, parts of the field illuminated from them will appear slightly darker than those illuminated from the center. If as shown in the diagram (Fig. 6) light from the central part of the lens of one collimator is matched from that of the edge of the other and *vice versa*, it is evident that a match cannot be obtained across the whole field. To remove this the central part of the field must be illuminated from the central part of the lenses of both collimators. It is desirable also that the light should pass centrally through the observing telescope and that the image of the field should not shift laterally as the telescope is rotated to bring other colors into the field. These conditions determine completely the relative positions of the prism, the axis of the instru-

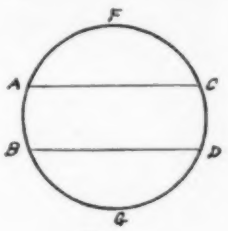


FIG. 3.

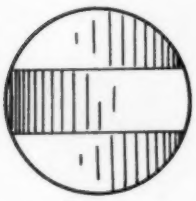


FIG. 4.

[1] H. v. HELMHOLTZ, *Physiologischen Optik*, pp. 289 and 301, 1896.

ment and the axis of collimation of the collimators and telescope. The axis of the instrument must pass through the image of the center of the silver strip as seen through the telescope and the axes of collimation of the collimators must meet the prism one-fourth way from the edge. With the instrument as first constructed it was impossible to meet these conditions fully without considerable alteration, and even when the instrument was so altered, the adjustment was inconvenient.

FIG. 5.

The new instruments have been so planned as to make the adjustment rapid and easy. A convenient size of prism (6 cm) was decided upon, and the position of the collimators, telescope and prism calculated for the glass used (n_D=1.65–1.66) and for minimum deviation for the sodium line. The collimators and telescope are mounted permanently on their arms in the required position. The prism is placed on a base having a projecting pin directly underneath the image of the center of the silver strip, which fits into a socket in the axis of the instrument. The details of the new design are given in Fig. 7. *O* is the vertical axis of the instrument, lying in the center of the image *AD'* of the silver strip *AD*. *Or* and *Or'* are the perpendicular distances of the axes of collimation of *T* and *T'*, respectively, from *O*. *O* lies a distance *AO* from the prism angle *A*, *Op* below the surface of the prism and *KO* from the center *K* of the prism. The side of the prism *AB* = 6 cm. *Or* = 1.8 cm. *Or'* = 2.6 cm. *AO* = 1.7 cm. *Op* = 0.1 cm. *KO* = 2.1 cm. *T* is fixed, while *T'* and *R* rotate about the axis

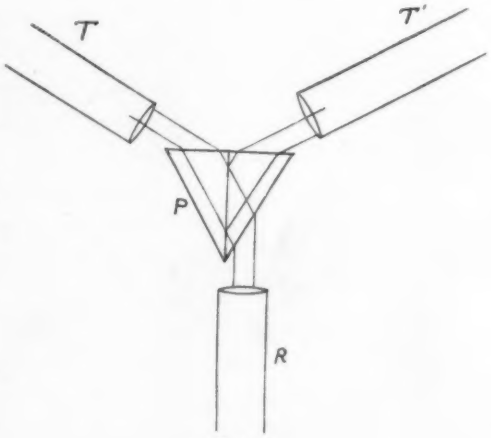


FIG. 6.

O, their positions being read on micrometer screws. With this arrangement all that is necessary for adjustment is to place the prism properly on the base, set it on the instrument, rotate it to minimum deviation for the image of the D line from the left hand collimator *T* and clamp. When the right hand collimator *T'* has been rotated about *O* till the two images of the sodium line coincide, the light passes symmetrically and centrally through the collimators and telescope. This arrangement obviously precludes the use of the instrument with prisms much different in size or varying much in refractive index from the glass chosen. The limit, however, is not narrow. Prisms as small as 5.7 or as large as 6.3 cm on a side could be used, though not so conveniently, and the refractive index may vary from 1.63 to 1.67 without seriously altering the adjustment. As the dispersion of the glass used is large enough for all ordinary work, the advantages of simplicity and ease of adjustment more than compensate for the limitations imposed by the design.

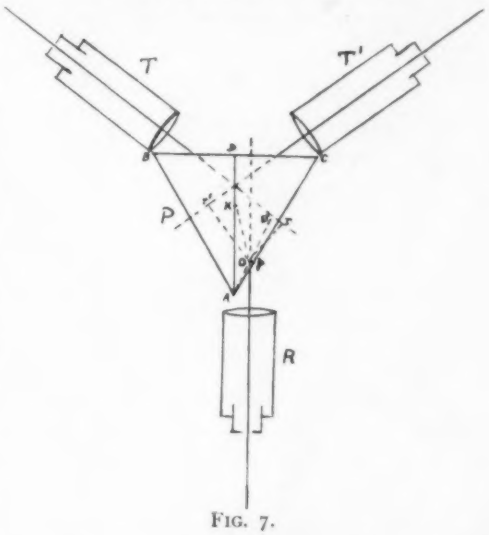


FIG. 7.

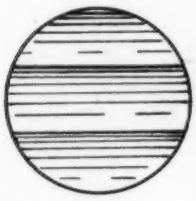


FIG. 8.

Another appearance of the field became noticeable when these adjustments had been made (Fig. 8), a match being obtainable only on one edge of the silver strip at a time. This might be due to unevenness in the silver strip, but the likelihood of that is small and the effect was never traced to that cause. A slight tilting of the prism on its base causes the light from the top of one slit to be matched with that from the bottom of the other and the same results as were found in lateral symmetry

were to be expected. When cross-hairs are placed across the center of the slits and the prism tilted till their two images coincide, the light comes from corresponding parts of the slits of both collimators, and the trouble disappears. Usually a strip of paper under one corner of the prism will correct this. With a uniform source of light the two fields will now appear evenly illuminated and a perfect match is obtainable, the dividing lines becoming almost invisible.

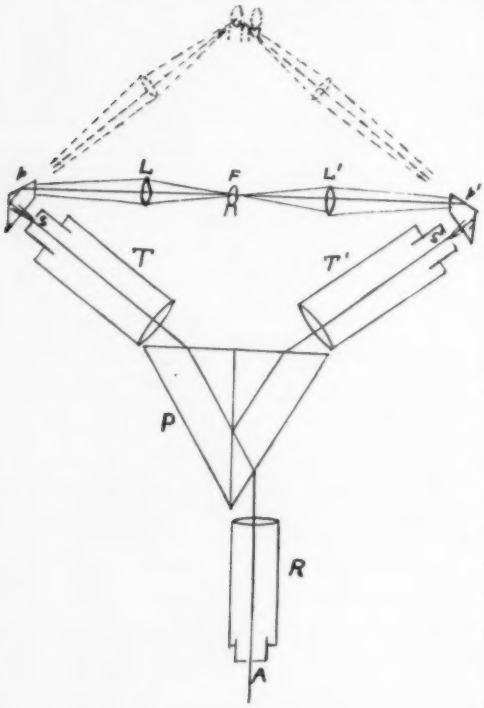


FIG. 9.

For the comparison of two sources of light, ground glass may be used, or if acetylene is made the standard of comparison the free flame is usually sufficiently uniform. For work on absorption the advantage of having both comparison fields lighted by the same source, and thereby avoiding errors due to variation in the sources, has long been recognized. It has, however, been difficult with mirrors and ground or opal glass to secure not only uniformity but even sufficient illumination to give the required sensibility. To avoid this loss of light the following arrangement (Fig. 9) was devised by Professor Brace. The light is taken from a flat flame F, and passes through the lenses L, L' by which after a double reflection at the prisms p, p' it is brought to a focus on the jaws of the slits S, S' of the collimators T, T', forming there images of the flame. The opposite sides of the same point of the flame serve as sources and if the light passes centrally through the lenses L, L' the field is evenly illuminated. The adjustment is

effected by placing a flame in front of the slit of the ocular telescope at A, then adjusting prisms and lenses until the light passes centrally through the lenses, and the images of the two slits S, S', thus illuminated, fall upon the same point of a screen which replaces the flame at F. As the light for both sides comes from the same point in the flame, comparisons are wholly free from disturbances caused by alterations in the intensity of the source. Flickerings which are readily seen in the flame produce no noticeable effect in the relative illumination of the fields. As there is usually an area of several square milimeters in a flame which has uniform intensity, a displacement of the flame from F towards L or L' has for quite a range of movement no effect upon the relative intensities of the two comparison fields, so that the variations which might be feared from that cause do not exist. The two prisms p, p' are exactly similar in shape, but p is silvered on one of its rear faces at the same time that the comparison prism is silvered. It is so cut that the light falls upon this silvered face at an angle of 60°, which is approximately the same angle as that at which the light is reflected from the comparison strip. This effectually compensates for the selective reflection of the silver strip, which would otherwise become noticeable in the blue by altering the reading for equal illumination of the two fields. The difference of setting for different colors sometimes runs as high as 2 or 3 per cent., but with a compensation prism p, a match for one color is, within one or two-tenths of 1 per cent., a match throughout the spectrum.

Several other arrangements were tried and abandoned, either because of lack of constancy or other faults. For instance the arrangement shown in the dotted lines (Fig. 9), in which the flame does not lie in the line joining the centers of the two lenses, was discarded because a slight lateral shifting would cause the light to be taken from different parts of the flame and produce marked variations in the relative intensity of illumination of the two fields. Both ground and opal glass were tried in the place of the lenses L, L', but uniform opal glass is very hard to obtain,

the surface of ground glass is easily ruined by handling, and, as mentioned above, both cut down the intensity so much (95 to 98 per cent.) that very intense sources are necessary to secure even moderate illumination of the field. The use of constant separate sources in absorption work was at first thought to offer some advantages, especially in greater intensity of illumination of the field, but although it was possible to prepare large and uniform incandescent filaments, which would not vary in intensity more than two or three-tenths of 1 per cent. in the course of an hour's work[1], they were expensive and fragile and gave no greater intensity than is now obtained by the system adopted.

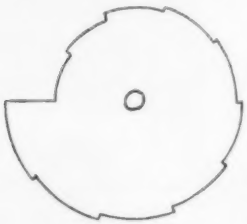


FIG. 10.

S' (Fig. 9) is a bilateral slit which is either used alone or in conjunction with a rotating sector for measuring the changes in intensity. If the slit is used alone it must first be calibrated optically[2] (preferably with a rotating sector) for various widths and different wave-lengths and for the source of light used, since owing to the curvature of the luminosity-curve the illumination is not, in general, proportional to the slit width.[2,3] The sector used is a cardboard or metal disk cut in a series of steps (Fig. 10) which divide the circumference equally, thus cutting out a definite fraction of the light at each revolution. By placing the different steps in succession before one slit, different fractions of the light are cut out, and by altering the width of the other (bilateral) slit to match, it can be completely calibrated.[2] When the instrument is in constant general use, the calibration will in the end save time, but where only a few observations are to be made, or where measurements are being made on strongly absorptive substances, the combination of sector and slit without calibration is often more advantageous, by saving time in the first case, and in the second by avoiding the large effect of zero

[1] E. V. CAPPS, ASTROPHYSICAL JOURNAL, 11, 25–35, 1900.

[2] *Phil. Mag.*, **48**, 420–430, 1899, and ASTROPHYSICAL JOURNAL, 11, 6–24, 1900.

[3] D. W. MURPHY, ASTROPHYSICAL JOURNAL, **6**, 1–10, 1897.

errors in reading the width of very narrow slits. The curves given by Mr. Capps[1] for flint glass show that if great accuracy is wanted, the proportionality of slit-width and intensity of field, especially in the red, is not to be relied upon for variations of intensity much exceeding 2 per cent. with a slit-width of 0.5 mm, so that in working with that width a sector of at least twenty steps would be necessary if the maximum error is to fall

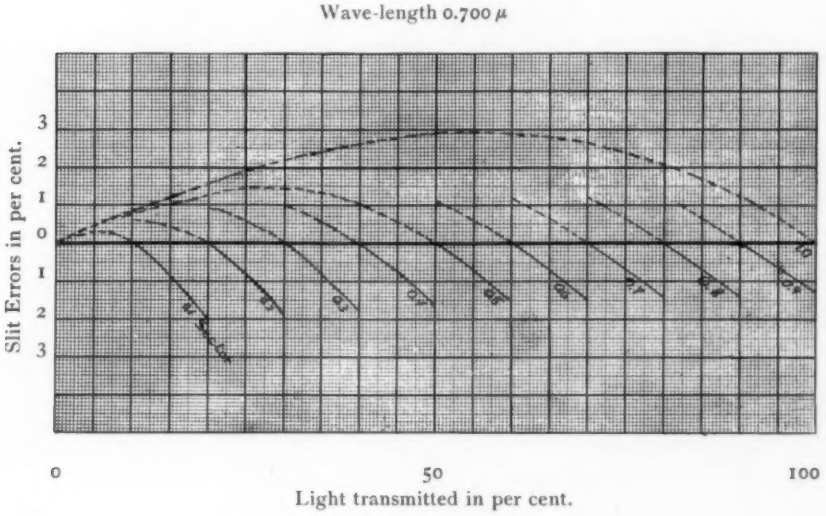


FIG. 11.

within 3 per cent. With the slit-width of 0.25 mm, a ten-step sector would give sufficiently accurate results. The variations of these errors due to lack of calibration for various intensity ratios is plotted from Mr. Capps' tables[1] for a ten-notch sector, a 0.5 mm slit and a wave-length of 0.700 μ, where he found the maximum error. The full lines give effects of decreasing, and the dotted lines of increasing the slit-width to obtain a match (Fig. 11). Of course in the measurement of strong absorption the sector can be used in conjunction with a calibrated slit, if high accuracy is needed. This method of simultaneous use of

[1] ASTROPHYSICAL JOURNAL, 11, 25–35, 1900.

sector and variable slit has been used by Professors Brace and Moore since 1899.[1] It has also been noted and its advantages pointed out by Brodhun[2] in a review of Professor Brace's article.[3]

UNIVERSITY OF NEBRASKA,
Lincoln, Sept. 23, 1902.

[1] D. B. BRACE, *Bull. Amer. Phys. Soc.*, 1, No. 2, pp. 29–31. Abstract of article read December 28, 1899.

[2] *Zeitschrift für Instrumentenkunde*, 20, 210–212, 1900.

[3] *Phil. Mag.*, 48, 420–430, 1899, and ASTROPHYSICAL JOURNAL, 11, 6–24, 1900.

THE PHYSICAL CAUSES OF THE DEVIATIONS FROM NEWTON'S LAW OF GRAVITATION.[1]

By PETER LEBEDEW.

THE question of the deviations from the general law of gravitation and their physical causes is one of the oldest questions of astrophysics — older, indeed, than Newton's law itself. It was propounded by Kepler three hundred years ago, and was answered by him in a way that we could not improve upon today. This deviation from Newton's law appears in its most striking and at the same time most simple form in the case of comets' tails, where it occurs as a pronounced repulsive force from the Sun. The development of the views as to the nature of this repulsive force and its basis in physics forms one of the most interesting chapters of astrophysics. The intimate association of an astrophysical theory with the current physical views and results can be followed in its course through three centuries.[2]

In 1608 Kepler expressed the opinion that comets' tails are the evaporizations of the heads, which move independently of the heads and are repelled by the Sun instead of being attracted. Kepler attributes the cause of this repulsive force to the solar radiation. The corpuscular theory of light was in his time the prevailing view in optics, and it is a necessary mechanical consequence of this theory that the rays of light should exert pressure upon the bodies they encounter, whence the repulsion of the very small particles of vapor by the solar rays could be explained. Newton[3] distinctly pointed out in 1687 that Kepler's assignment of the repulsion to the forces of pressure of the radiation might

[1] A paper presented at the Göttingen meeting of the *Astronomische Gesellschaft* on August 4, 1902, communicated by the author.

[2] An extended account of the history of this development may be found in DE MAIRAN'S *Traité physique et historique de l'Aurore Boréale* (Second Edition), Paris, 1754; and in Zöllner's *Wissenschaftliche Abhandlungen*, Zweiter Band, Zweiter Theil. Leipzig, 1878.

[3] *Principia*, Lib. III. London, 1687.

be assumed as a method of explanation, but he did not himself adopt Kepler's view, attempting to bring the repulsion of the comets' tails under his law of gravitation and considering it merely as an apparent repulsion. He made the hypothesis that the universe is filled with a gaseous medium denser than the gases of the comets' tails, which therefore are buoyed up (according to the law of Archimedes) and are therefore only apparently repelled by the Sun.

Euler[1] in 1744 clearly perceived the difficulties associated with Newton's hypothesis and returned to Kepler's view, similarly attempting to explain the repulsion by the pressure of light. But as an active opponent of the emission theory of light, Euler[2] had adopted the Huyghens theory and assumed that light is an undulatory motion with longitudinal displacements in the ether. He treated the matter as a series of mechanical impacts which the incident longitudinal waves exert upon the bodies affected, and in this way accounted for the existence of the pressure of light.

In the middle of the eighteenth century, some thirty-five years before Cavendish investigated in the laboratory the law of attraction, de Mairan and du Fay (1754) made the first attempt to test experimentally the pressure of radiation which should cause the deviation from Newton's law. De Mairan and du Fay[3] planned their research with admirable skill, but they encountered difficulties (air currents) which could not be overcome with the experimental means of the eighteenth century, and they were compelled to leave unanswered the question of the existence of the mechanical pressure of light.

Attention was directed in the nineteenth century to the motion of comets by the pioneer investigations of Olbers. He regarded the repulsive force on comets' tails as a fact firmly established by observations; as to its physical foundation, Olbers[4] in 1812

[1] See ZÖLLNER, *loc. cit.*, p. 525.

[2] *Histoire de l'Academie Royale de Berlin*, 2, 121, 1746.

[3] DE MAIRAN, *Traité*, *loc. cit.*, p. 371.

[4] *W. Olbers Leben und Werke*, 1, 331. Berlin, 1894.

discarded the views of Kepler as well as those of Newton as hypotheses which could not be based on experiment, and he expressed a new suspicion in a very cautious way: "One can scarcely refrain from thinking of something analogous to our electrical attractions and repulsions." If we reflect that Olbers expressed this suspicion as the theory of electricity was celebrating its first triumphs, we can comprehend his suggestion of electric forces, the laws of whose action were known from the direct experiments of Coulomb (1785).

The electrical theory of Olbers now became prevalent. The decrease of the electrical force with the square of the distance (which also applies to the pressure of light) sufficed for Bessel [1] to give in 1836 a simple theory of comets' tails, and to compute the absolute magnitude of the repulsive force from the curvature of the tail. From the measurement of many comets' tails, Bredichin[2] found the magnitude of the repulsive force to be characteristic for different component substances in the tail, and he determined its value to be 0.2, 1.1, and 17.5 times the force of attraction.

The electrical theory of Olbers depends upon two hypotheses —first, that the Sun has a constant electrical charge, and second that the separate molecules of the gases of the tail receive charges upon leaving the head, the sign of which agrees with that of the solar charge. No progress worthy of mention has been made in the course of years in establishing a physical basis for these two hypotheses. The assumption that the Sun is a charged body could be connected with the magnetic phenomena of the Earth only by introducing additional hypotheses; but the absolute magnitude of the solar charge could not be measured, indeed not even its sign could be determined. Phenomena of electrification of the separate molecules of gas under the conditions assumed by the second hypothesis have not been discovered by the physicist in the laboratory.

The similarity of the optical phenomena in comets' tails and in Geissler tubes is often invoked in support of the assumption

[1] *A. N.*, 13, 185, 1836. [2] *Annales de l'Observatoire de Moscou* (2), 1, 45, 1886.

of an electrification of the materials of the tail, but such an argument is unpermissible and contradicts the principle of the conservation of energy, according to which every phenomenon of luminosity is associated with the giving out of energy, which is not possible for molecules of gas having a constant *electrostatic* charge. The cause of the luminosity of comets' tails is probably to be attributed to the fluorescence of strongly illuminated gases, which has been demonstrated by the direct experiments of Lommel[1] and of Wiedemann and Schmidt[2].

The serious objections which may be raised against the electrical theory were pointed out by Zöllner[3] (1872), who most fully worked out the electrical theory, but declared himself ready to give up his theory and adopt that of Kepler if the existence of a pressure of solar radiation could be proven.

The question of the existence of such a pressure was solved independently of astronomical theories thirty years ago, by Maxwell[4] (1873) as a result of his electro-magnetic theory of light, and by Bartoli[5] (1876) as a consequence of the second law of thermodynamics. These theoretical investigations agreed in indicating that the pressure (p) of radiation must necessarily exist, and that it stands in a simple relation to the quantity of energy (E) falling upon the body per second, as a beam of parallel rays, and to the velocity of light (v). For an absorbing body this relation is

$$p = \frac{E}{v} .$$

For the solar radiation at the distance of the Earth this gives a pressure of 0.5 milligram per square meter. It has recently been possible for me[6], as well as for Nichols and Hull[7], to

[1] *Wied. Ann.*, **19**, 356, 1883.

[2] *Wied. Ann.*, **56**, 18, 1895; **57**, 447, 1896.

[3] *Ueber die Natur der Kometen*, p. 198. Leipzig, 1872.

[4] *Treatise on Electricity and Magnetism*, § 792.

[5] *Il Nuovo Cimento*, **15**, 195, 1883.

[6] *Ann. der Physik*, (4), **6**, 433, 1901; ASTROPHYSICAL JOURNAL, **15**, 60, 1902.

[7] *Physical Review*, **13**, 307, 1901; ASTROPHYSICAL JOURNAL, **15**, 62, 1902.

demonstrate the existence of this pressure of light by direct laboratory experiments, and the formula due to Maxwell and Bartoli was quantitatively confirmed.

Before these experimental researches had been made, Fitzgerald[1] in 1884 applied the theoretical results of Maxwell to the deviations from Newton's law in the case of the motion of comets, but he committed the error of extending his results to the gaseous molecules of the tail without considering that Maxwell's deduction only holds for bodies which are large in comparison to the wave-lengths of the incident radiation. This error was avoided in the considerations simultaneously advanced by Lodge[2] and by myself[3] as to the repulsive force of the Sun, and by myself[4] as to the deformation and disintegration of comets' heads. In his theory of the solar corona Arrhenius[5] has recently made the same mistake again.

For a spherical body of large dimensions as compared with the wave-lengths of the solar radiation, the resulting action (F) of the Sun is expressed in terms of the force of gravitation as

$$F = 1 - \frac{1}{10{,}000} \cdot \frac{1}{r\delta},$$

where r is the radius in centimeters and δ is the density of the body (referred to water as unity)[6]. It is evident from this that the deviations from Newton's law for a body whose dimensions exceed one meter are far less than the limits of error of the most delicate astronomical observations.

For the head of a comet consisting of a swarm of stones smaller than 1 cm, the deviation from the law of gravitation can just be proven under favorable conditions of observation; for still smaller stones the deviation will be correspondingly

[1] *Proc. Roy. Soc. Dublin*, 1884. [2] *Nature*, 1891.

[3] *Wied. Ann.*, **45**, 292, 1892. At that time I had unfortunately overlooked Fitzgerald's paper.

[4] *Rapports présentés au Congres internationale de Physique à Paris*, **2**, 133, 1900.

[5] *Physikalische Zeitschrift*, **2**, 81, 1900.

[6] See *Wied. Ann.*, **45**, 294, 1892. The Sun's rays exert a somewhat greater repulsion upon bodies which are not spherical, as for such the ratio of surface to volume is greater.

larger. The converse proposition may however be asserted — that if there is no appreciable deviation from Newton's law, and if the extent of the uncertainty of the observations is known, the lower limit can be assigned for the size of the stones of the comet's head.

If the head consists of a swarm of meteorites of unequal sizes, some sufficiently small, the swarm will be deformed and will disintegrate — as must be especially conspicuous in the case of comets which have become periodic. The prediction, in the ordinary manner, of the orbit of such a swarm will show greater departures from the subsequent observations. It is possible that the peculiar movements of the Bielids can be explained in this way.

The formula cited above fails for the case of dust particles with dimensions of the thousandths of a millimeter — which are therefore comparable with the wave-lengths of the incident solar radiation. Schwarzschild[1] has shown that in this case the repulsive force reaches a maximum at certain dimensions and then rapidly decreases for smaller dimensions.

Gaseous molecules which are exposed to the solar radiation develop resonance phenomena, which are accompanied by forces of pressure from the incident rays, as I have shown by computations[2]. But in this phase of the subject, which is of especial importance in astrophysics, we must still await the results of direct experimental researches.

As we review the historical development of our views as to the physical cause of the deviations from Newton's law of gravitation, we see that the view of Kepler, after being displaced first by Newton's pressure theory and then by Olbers' electrical hypothesis, again appears, now based upon the pressure of light theoretically deduced by Maxwell and Bartoli, and demonstrated by direct experiment. Kepler's theory has received a physical

[1] *Berichte der Münchener Akad.*, 1901.

[2] *Wied. Ann.*, **62**, 170–172, 1897. For gases the amount of the pressure is equal to the quantity of energy absorbed divided by the velocity of light. The ratio of absorption to mass is very different for different gases.

foundation, and we are now obliged to assert that the Sun does possess repulsive forces, the magnitude of which is obtained from the laboratory experiments. We can now quantitatively predict in advance the deviations from Newton's law which must necessarily occur, or we can discuss the consequences of them.

The question whether electrical actions may arise in one case or another, also carrying with them an appreciable departure from Newton's law, must at present be left as an open question. Not until we have taken into quantitative account the action of the pressure of light, which is unquestionably present, can we conclude as to the presence or absence of further forces, and assert whether additional assumptions are necessary or whether that of Kepler is alone sufficient.

MOSKAU,
August, 1902.

NITROGEN BANDS *VS.* "NEW HEADS TO CYANOGEN BANDS" IN ARC SPECTRA.

By PERCIVAL LEWIS and A. S. KING.

IN the June number of this JOURNAL Professor C. C. Hutchins[1] published a reproduction of a photograph of the spectrum of the arc between copper electrodes, in which appears a band with its head at λ 3914.47, which he assumes to be a new head to the group of cyanogen bands beginning at λ 3883.55. He found another head at λ 4278.5, and possibly one above λ 4606, which he thinks may bear a similar relation to the cyanogen bands at λ 4216 and λ 4606.

Deslandres[2] has shown that the most prominent of the negative-electrode bands of nitrogen has its head at λ 3914.6, and Hasselberg[3] places the head of another of these bands at λ 4278.6. Their general appearance is not unlike that of the cyanogen bands, except for the very characteristic property, pointed out by Deslandres,[4] that the lines composing these negative-electrode bands are alternately strong and weak in the neighborhood of the heads. This peculiarity is strikingly shown on Professor Hutchins' plate.

The writers have measured the wave-lengths of all the lines on the plates of Hutchins and Deslandres between the limits λ 3914 and λ 3883, with the exception of those crowded together in the head, and the results are given below. As these measurements are taken from prints on ordinary paper, the individual results are liable to considerable error, but the general agreement between the parallel columns is very striking. The results are not selected, but all the lines between the limits are given, and there is a line-for-line correspondence. The identity of the two

[1] ASTROPHYSICAL JOURNAL, **15**, 310, 1902.

[2] *C. R.* **103**, 375, 1886.

[3] *Mem. de l'Acad. St. Petersburg*, 1885 (7), **32**, No. 1.

[4] *Ann. Chim. et Phys.* (6), **15**, 57, 1888.

bands is shown very clearly by making a diagonal scale from one plate and comparing it directly with the other.

Hutchins Copper arc in air	Deslandres Nitrogen, negative pole	King Carbon arc, Nitrogen atmosphere	Hutchins Copper arc in air	Deslandres Nitrogen, negative pole	King Carbon arc, Nitrogen atmosphere
3914.47	3914.60	3914.55	3904.55	3904.45	3904.61
14.00	13.95	....	03.61	03.35	....
13.55	13.50	....	02.55	02.35	....
13.32	13.20	13.10	01.47	01.30	....
12.90	12.85	12.76	00.35(?)	00.05	....
12.62 }	12.40	....	3898.94	3898.85	....
12.17 }		....	97.69	97.60	....
11.75	11.8	....	96.30	96.35	....
11.13	11.20	....	95.18	95.00	....
10.85	10.60	....	93.70	93.65	....
10.06	10.00	....	92.30	92.20	....
09.45	09.40	09.43	90.80	90.80	....
08.65	08.60	....	89.45	89.30	....
08.00	07.85	....	87.75	87.70	....
07.14	07.10	06.80	86.20	86.10	....
06.40	06.25	06.46	3884.40	3884.45	....
3905.55	3905.35	....			

On Hutchins' plate there is the appearance of an overlapping band beginning at about λ 3889, and below that point several lines seem double. This may have slightly affected some of our measurements.

On some of the plates taken by one of us[1] while studying the cyanogen bands in the carbon arc, some peculiar shadings and grouping of lines attracted our attention. This was especially marked near the head of the λ 3883 band, particularly on some plates taken with the arc in a nitrogen atmosphere. Some faint lines were present on these plates and absent from others of equal exposure and development. It seems unlikely that they were due to metallic impurities; the carbons were prepared from calcined crystallized sugar, and showed scarcely a trace of any impurity. The wave-lengths of such of these lines as appeared particularly differentiated from the carbon lines in the neighborhood were measured, and the results are given in the third column of the above table. The carbon lines are crowded so closely in this region that it would not be difficult

[1] A. S. KING, ASTROPHYSICAL JOURNAL, **14**, 323, 1901.

to find almost any desired wave-length; but it is to be noted that these extra lines appearing on some plates only were first selected by inspection, and their wave-lengths afterward determined quite independently of the numbers in the first two columns; there was no selection or elimination. On some plates the line at λ 3914.6 is very sharply defined, on others entirely absent. The results seem to make it appear at least possible that this band of the negative-pole nitrogen spectrum may be found even in the carbon arc.

In the light of these measurements it seems probable that Professor Hutchins has made a mistake in his conclusions; but at the same time he has observed a very interesting and important fact, for, so far as known to the writers, no one has ever before found in the arc spectrum the bands or lines of any permanent elementary gas except those of hydrogen.

Several facts besides the identity of wave-lengths with those of the nitrogen band oppose the view that this band is due to cyanogen. It has never been observed before, notwithstanding all the varied conditions under which the cyanogen spectrum has been studied. Professor Hutchins found that moistening the copper electrodes with oil greatly intensified the band at λ 3883, but had no effect on that at λ 3914, as one might expect if this were a cyanogen band. The physical appearance of the lines is also different; in the photograph the cyanogen lines run across the field, while the lines of the other band are confined to the neighborhood of one pole — presumably the negative.

On the other hand, some analogies with spark spectra make it seem reasonable to assume the possibility of the appearance of the nitrogen spectrum under the special conditions of Professor Hutchins' experiment — namely, with a hissing or discontinuous arc, which is kept cool by periods of extinction and by the high thermal conductivity of copper. The fact that on starting the arc the negative pole was tipped with a bright glow would further lead us to look particularly for the negative-pole bands. We should certainly expect to see nitrogen bands or lines in the spectrum of the transformer discharge between

terminals of nickel or cobalt, in which cases Hutchins observed the band at λ 3914, while the cyanogen bands were absent.

The bands were most clearly brought out by intermittently starting and stopping the arc, thus keeping it cool and reducing the amount of metallic vapor present. It is to be noted that the copper lines in the plate are stronger near the hot positive electrode, and grow fainter as they approach the cooler negative pole; the air seems to pick up the current at the point where the metallic vapor fails, and to become luminous in consequence. In spark spectra we see similar effects; the metallic lines are stronger at the poles, where the vapor is denser, while the air bands or lines bridge the cooler gap. In the case of the spark discharge it has also been shown[1] that the first partial discharge passes through air; the succeeding sparks go by preference through the metallic vapor produced by the first spark. If the same be true of the arc we can see why the nitrogen bands are brought out more strongly by the intermittent arc.

The writers have endeavored to find air bands visually in the spectrum of the arc, between copper electrodes, but without success. The band at λ 3914 is too near the limits of the visible spectrum to expect to see it. Deslandres states that this band is the strongest in the negative-electrode spectrum of nitrogen, and its occurrence in the arc while all other parts of the nitrogen spectrum (except the fainter band at λ 4278) are absent, indicates that of all the spectra of nitrogen that of the negative pole appears under the lowest conditions of temperature or current intensity.

It seems as though this band should be found on some of the plates taken by Professor Crew with the rotating metallic arc, when some of the less volatile metals were used.

UNIVERSITY OF CALIFORNIA,
Berkeley, August 20, 1902.

[1] SCHUSTER and HEMSALECH, *Phil. Trans.*, 193, 189, 1899.

THE TOTAL LIGHT OF ALL THE STARS.

By GAVIN J. BURNS.

MY attention has been called to the article in your issue of December last on the above subject. Professor Newcomb there observes that he cannot learn that any attempt to measure the amount of light received from various regions of the sky has ever been made. As I have made a number of observations on this subject during the last four years, in the course of which I have gone over nearly the same ground as Professor Newcomb, I venture to send a short account of them.

1. *Relative brightness of different portions of the sky.*—My attempts to measure the relative brightness of the sky have not been very successful. Like Professor Newcomb, I was much surprised at the smallness of the difference between the brightness of the galaxy and the rest of the sky. My first attempt was based on the principle of Abney's revolving sectors. The brighter part of the sky was viewed through an aperture in front of which a disk with openings in it was made to revolve rapidly, while the less bright part was viewed through a similar aperture but without the revolving disk. I obtained no results of any value by this method.

I next tried the following plan: I procured several pieces of ordinary clear glass about 10cm by 7cm. I then looked at the part of the Milky Way I wished to examine through one, two, three, or four thicknesses of glass, and compared the brightness of the Milky Way seen through the glass with some other portion of the sky. The result was that it required four thicknesses of glass to reduce the light of the Milky Way near *Orion* to the general luminosity of the extra-galactic sky, while it required seven thicknesses to produce the same results in the brightest part of the region between *Cassiopeia* and *Cygnus*. I have been unable to detect any difference in the luminosity of the sky outside the visible boundary of the Milky Way. I next

determined by the best means at my disposal the proportion of light transmitted by the glass. This I found to be 52 per cent. for four thicknesses and 36 per cent. for seven thicknesses. It follows that the luminosity of the Milky Way is from two to three times as great as that of the rest of the sky.

2. *Amount of sky light compared with star light.*— The method I adopted was to compare the brightness of the sky with the brightness of a star which was out of focus in a telescope. I used a 9.5 cm refractor with a power of 100. A suitable star was selected and put out of focus to such an extent as to make the brightness of the apparent disk equal to the brightness of the sky. The latter was viewed through a hole in a black screen which subtended an angle roughly equal to the angular diameter of the disk of the star. One eye was applied to the telescope and the sky viewed with the other. Each observation recorded is the mean of a right-eye and a left-eye observation. The following table gives the results:

Name of star	Magnitude H. P.	Angular diameter of disk
μ *Leonis*	4.14	12°
ζ *Ursae minoris*	4.49	11
δ *Ursae majoris*	3.41	17
β *Eridani*	2.87	17
κ *Geminorum*	3.61	11.5

The last column gives the apparent diameter of the star when put out of focus to such an extent as to equal the light of the sky. Let D = this quantity.

Also, let a = aperture of telescope,
e = aperture of pupil of eye,
b = fraction of light, transmitted by telescope,
x = area of sky that gives an amount of light = that of star of magnitude m;
y = Ditto = that of star of magnitude o.

Then
$$x = \left(\frac{De}{a}\right)^2 \frac{\pi}{4b}$$

and
$$y = x \times 10^{\frac{2m}{5}}.$$

In order to determine x, the values of e and b must first be found. I measured e by finding the diameter of an opaque disk which would stop all the light coming from a small distant hole. I found it to be between 6.6 mm and 7.1 mm (0.26 and 0.28 inch). I assumed $e/a = 0.07$. I determined b by turning the telescope to the Sun and allowing its image to be formed on a sheet of white paper, which was put at such a distance as to give an image of the same brightness as the part of sheet exposed to the direct rays of the Sun. The image was then 6.35 cm in diameter. Consequently

$$b = \left(\frac{6.35}{9.53}\right)^2 = \frac{4}{9}.$$

The results are as follows:

μ *Leonis* gives	$y = 56.4$
ζ *Ursae minoris* gives	$y = 65.4$
δ *Ursae majoris* gives	$y = 57.8$
β *Eridani* gives	$y = 35.1$
κ *Geminorum* gives	$y = 31.9$
Mean	$y = 49.3$

Or half a square degree of non-galactic sky gives as much light as a star of magnitude 5.0.

I believe the discordance in the above observation is largely due to the difference in color between the sky and the image of the star in the telescope, and the consequent difficulty in making a comparison. The object-glass of my telescope is of a perceptibly greenish tinge. I believe better results could be obtained with a smaller telescope having a thinner object-glass, or with a reflector.

3. *Faintest magnitude visible.*—In the course of the observations above recorded I discovered, somewhat to my surprise, that the faintest stars visible to the naked eye could be seen through a telescope with the aperture cut down to 3 mm (0.125 inch). Allowing for loss of light in the telescope itself, only about one-tenth of the light reaches the eye through this aperture which would enter the unaided eye. It appears, therefore that on a perfectly dark background stars of the eighth magnitude would be readily visible.

KENDAL HOUSE, HOLLAND ROAD,
WEYMOUTH, ENGLAND.
September 2, 1902.

COÖPERATION IN OBSERVING RADIAL VELOCITIES OF SELECTED STARS.

By EDWIN B. FROST.

IT is a somewhat peculiar fact that in the present state of spectrographic work it is not possible to find published determinations of the radial velocities of as many as a dozen stars (a few spectroscopic binaries excepted) which have been observed at three different observatories. This anomalous condition is probably in part due to the fact that in the past two or three years most of the large spectrographs designed for line of sight determinations have been in process of construction or reconstruction; and perhaps in part also to the disinclination of the observers to publish their results while subject to uncertainties as to the wave-lengths of comparison lines or stellar lines.

For those not engaged in work of this kind, probably the most satisfactory evidence of the accuracy of the determinations would be gained from a comparison of the results of different observers and instruments. And while each observer doubtless satisfies himself of the accuracy of the operation of his instrument by frequent measurements of the velocity of the Moon and planets, nevertheless there would be great satisfaction in finding that the different observers were obtaining substantially identical results, although using different comparison spectra, different stellar lines, and with generally different conditions of observation, measurement, and reduction. The history of progress in other fundamental measurements would suggest that systematic differences between the results at different observatories are likely to become more apparent as the accuracy of the determinations increases. The limit of accuracy ordinarily attainable at present in determining the velocity of approach or recession of a star of the solar type with sharp spectral lines is not known, and cannot be satisfactorily established by comparing

observations made with one instrument, in which there may be present sources of systematic error, however small.

For reasons like these and for others which have doubtless often occurred to spectroscopists, it would seem highly desirable to have some common action by different observers which would furnish a basis for an estimate of the accuracy of spectrographic work in general, and of that of each observer and instrument in particular.

It is therefore very satisfactory to be able to report that a beginning has been made with a simple plan of coöperation in the observation of the radial velocities of a few selected stars, in which it seems probable that the observers with the principal spectrographs now in use will take part as far as possible. The present status of the scheme can perhaps be best indicated by quoting here two circular letters recently sent out by the writer.

YERKES OBSERVATORY, FEBRUARY 20, 1902.

DEAR SIR: For reasons briefly stated in a paper in the January number of the ASTROPHYSICAL JOURNAL, a reprint of which accompanies this letter, it has seemed to the writer that the time has arrived for an attempt at coöperation in the observation of certain stars (which might be called "fundamental velocity stars") by the spectrographs now engaged in such research.

Opinions may differ as to the present desirability of such coöperation and as to its details; hence the principal object of this letter is to inquire whether, and under what circumstances, you would be prepared to coöperate in the observations.

In order to expedite arrangements, should it prove that several spectrographs will coöperate, a provisional list of stars is submitted for your consideration, in the hope that you will freely suggest any changes in the list, or propose still other stars.

In the preparation of the list attention has been given to the distribution of the stars in R. A. and Dec., to the character of their spectra, and to their magnitude. Spectra of nearly all of these stars have been obtained at the Yerkes Observatory, and it is believed that the lines will in all cases be found suitable for accurate measurement. Stars known to be visual[1] or spectroscopic binaries and those varying in brightness have been excluded from the list.

Although the time required for merely obtaining the plates of these spectra will not be significant, yet in view of the expenditure of time neces-

[1] *β Leporis* excepted.

PROVISIONAL LIST OF TWENTY "FUNDAMENTAL VELOCITY STARS."

Star	R. A.	Dec.	Mag.	Class
β Cassiopeiae	$0^h\ 04^m$	$+58°\ 36'$	2.4	XII *ab*
α Arietis	2 02	$+23\ 0$	2.0	XV *a*
α Persei	3 17	$+49\ 30$	1.9	XII *ac*
α Tauri	4 30	$+16\ 19$	1.0	XVI *a*
β Leporis	5 24	$-20\ 51$	3.0	XIV *a*
γ Geminorum	6 32	$+16\ 29$	2.0	VIII *a*
β Geminorum	7 39	$+28\ 16$	1.1	XV *a*
β Cancri	8 11	$+9\ 30$	3.8	XV *a*
α Hydrae	9 23	$-8\ 13$	2.2	XV *a*
α Crateris	10 55	$-17\ 46$	4.1	XV *a*
β Corvi	12 29	$-22\ 50$	2.8	XIV *a*
α Boötis	14 11	$+19\ 44$	0.0	XV *a*
α Serpentis	15 39	$+6\ 44$	2.7	XV *a*
β Ophiuchi	17 38	$+4\ 36$	2.9	XV *a*
η Serpentis	18 16	$-2\ 56$	3.4	XV *a*
γ Aquilae	19 42	$+10\ 22$	2.8	XV *a*
ε Cygni	20 42	$+33\ 36$	2.7	XV *a*
ε Pegasi	21 39	$+9\ 25$	2.4	XV *a*
α Aquarii	22 01	$-0\ 49$	3.2	XIV *ac*
γ Piscis	23 12	$+2\ 44$	3.8	XV *a*

The classification of Miss Maury has been used above.

sary for measurement, the list of twenty stars may seem too long, if each one is to be observed three times annually with each coöperating spectograph. To the writer it would seem sufficient, at the beginning, if the list should be limited to ten or twelve stars.

It is, of course, desirable that the same comparison spectrum should be employed as is regularly used with the given spectrograph; variety between the different spectrographs being desirable. Agreement should be reached as to the number of star lines to be measured on a plate, and the suggestion is made that the number should not exceed twenty; but it would seem undesirable to make any effort that the same lines be used with the different instruments.

Awaiting with interest your reply, and hoping for your coöperation, and an expression of your views, I am, etc.

YERKES OBSERVATORY, JUNE 26, 1902.

DEAR SIR: The circular letter in reference to regular spectographic observations of certain selected stars, of date of February 20, was addressed to Messrs. Bélopolsky, of Pulkowa, Campbell, of Mt. Hamilton, Deslandres, of Meudon, Gill, of Capetown, Lord, of Columbus, O., Newall, of Cambridge, England, and Vogel, of Potsdam. Replies have not yet been received from Messrs. Gill and Newall, but from all the others letters have now arrived, in

general favorable to the suggestions of the circular letter, It therefore seems desirable to communicate without further delay the substance of the responses that have been received, so that some degree of coöperation may be begun as soon as possible.

Taking up the letters in order, a condensed statement may be made as follows:

I. A. BÉLOPOLSKY (dated March 18, 1902): "Ich nehme gewiss gerne Theil in der von Ihnen vorgeschlagenen Arbeit." It would be well (1) if a general scheme of the observations could be worked out, covering, for instance, points like these: Should each star be observed in each position of the instrument (camera above, camera below) or in only one? Should the comparison spectrum be photographed at the beginning and end or at middle of exposure to star? (2) While leaving the choice of a comparison spectrum open to each observer, it would be interesting to have an additional comparison spectrum, as hydrogen, common to all observers; making the exposure to the hydrogen tube, for instance, at the middle of the star's exposure. (3) The star list should be longer, and stars with large velocity are desirable. (Stars below the equator could hardly be observed at Pulkowa.) (4) Epochs of observation should be arranged to eliminate errors of reduction to Sun, making the reduction once positive, and once negative. (5) As possible substitutes for the southern stars the following are suggested: Beta *Andromedae*, Mag. 2.3; Iota *Aurigae*, 3.0; Epsilon *Leonis*, 3.2; Epsilon *Virginis*, 3.0; Beta *Boötis*, 3.0; Delta *Boötis*, 3.0; Zeta *Herculis*, 3.0; Gamma *Cephei*, 3.5.

II. W. W. CAMPBELL (June 14): "It gives me pleasure to say that I think the scheme is an excellent one in its general features, and the Lick Observatory will be glad to coöperate in carrying through a well-considered program. The subject is one whose importance impressed itself upon me a few years ago, and was the subject of numerous conversations between Professor Keeler, Mr. Wright and myself. . . . . It seems to me that ten stars are sufficient, and that twelve should be the maximum, three observations each per annum." He suggests that the following ten stars be omitted from the list: Beta *Cassiopeiae* (on account of its poor lines), Alpha *Tauri*, Gamma *Geminorum*, Beta *Cancri* Alpha *Hydrae*, Beta *Corvi*, Alpha *Serpentis*, Eta *Serpentis*, Epsilon *Cygni*, Alpha *Aquarii*. "In past years we have secured a considerable number of observations of all the twenty stars on your list, so that we have a good starting point for all of them. (June 19.) Further consideration of the matter convinces me that thirty of these standard plates per year as a maximum will answer all purposes fully as well as sixty."

III. H. DESLANDRES (March 14): "Votre idée d'une coöperation de tous les observatoires est excellent, et je l'accepte immediatement." Ten stars, suitably distributed, would be sufficient, at least for the beginning. To

render the observations fully comparable, it would be well to designate for each star a period of ten days within which the participators should make one or two spectrograms of that star.

IV. H. C. LORD (February 29): The list should be of ten or twenty stars, and each should be observed on five nights, then published. After this work had been completed each observer could use the list as he chose, preferably taking one of them on each observing night as a control star. Thirty spectrograms of one star would be more desirable than three spectrograms annually of ten stars.

V. H. C. VOGEL (March 1902): "In Beantwortung Ihres Schreibens vom Februar 1902, theile ich Ihnen mit dass ich Ihren Vorschlag dass eine Anzahl von Sternen von allen denjenigen Beobachtern, die sich mit spectrographischen Geschwindigkeitsbestimmungen beschäftigen, regelmässig jedes Jahr beobachtet werden soll, für ganz zeitgemäss halte, und dass ich schon früher den Gedanken gehabt habe einen ähnlichen Vorschlag zu machen." It is recommended that the number of fundamental stars be made as small as possible, but that they should be observed more than three times yearly. The stars Alpha *Persei* and Alpha *Boötis* are suggested, as they can be consecutively observed for a long time by northern observers. From its high declination Alpha *Persei* offers opportunity for measurement at very different hour angles. Ten star lines would be sufficient for a plate, and observers would not be expected to exceed twenty star lines. For the sake of uniformity in publication it is suggested that the following data should be given: (1) Place and date. (2) Greenwich Mean Time, for the middle of the exposure time. (3) Value of the linear dispersion in some clearly intelligible form, using either $\mu\mu$ per $\frac{\text{mm}}{4}$ or tenth-meters per mm. (4) Velocity of the star in kilometers referred to the Sun, reduced with Schlesinger's tables: (*a*) Plate so placed on microscope carriage that increasing readings correspond to increasing wave-lengths; (*b*) Plate in the opposite direction. (Note. It would be interesting to have the results of the two series of measures given separately.) (5) Velocity of the star in kilometers referred to the Sun, the mean of the results (*a*) and (*b*). (6) Mean error, $\epsilon = \pm\sqrt{\frac{\Sigma v^2}{n-1}}$, in kilometers, of the determination of the velocity from a single line. The mean of the separate settings of the micrometer thread should be taken as the value for the line, and in both positions of the plate under the microscope. (7) Number of lines (m) measured in the spectrum. (8) Mean error, $\epsilon = \pm\sqrt{\frac{\Sigma v^2}{m\,(n-1)}}$ in kilometers of the final velocity of the star deduced from one plate. It is desirable that the observers should give once for all the values employed for the wave-lengths in the comparison spectrum and in the stellar spectrum, as for

instance: comparison spectrum, iron, wave-lengths according to Kayser; stellar lines: wave-lengths according to Rowland's tables for the solar spectrum.

VI. E. B. FROST: To these expressions of opinion may be added those of the circular letter of February 20, as setting forth the views of the writer, which have the approval of the director of the Yerkes Observatory.

It would now appear possible to find in the statements for the six spectrographs concerned a sufficient basis for beginning a scheme of coöperation, the incidental details of which may be worked out subsequently and gradually.

It seems to express the average opinion (1) that a list of ten stars should be adopted; (2) that three or more observations should be secured annually of each of these; (3) that a greater number of observations should be secured of Alpha *Boötis* and Alpha *Persei;* (4) that the observations of the other eight should be suitably distributed during the year, and should be made as nearly as convenient at the same dates by the different observers; (5) that the results should be published in a uniform manner.

From a consideration of what has *not* been stated in the responses, it would seem reasonable to draw certain inferences, *e.g.*, (*a*) that large latitude should be reserved to each observer in his mode of observing, choice of comparison spectrum, selection of star lines, and method of reduction; (*b*) that the suggested coöperation should not involve so much observation and reduction as to be a burden upon the participators and interfere with their regular programs of work.

Referring, then, to the summary of opinions, it would seem well: 1. To adopt as the star list that previously submitted, with the omissions suggested by Professor Campbell. This would accordingly include:

Alpha *Arietis*	Alpha *Boötis*
Alpha *Persei*	Beta *Ophiuchi*
Beta *Leporis*	Gamma *Aquilae*
Beta *Geminorum*	Epsilon *Pegasi*
Alpha *Crateris*	Gamma *Piscis*

For those who could not well secure the southern stars, substitutes could be chosen, if desired, from Professor Bélopolsky's supplementary list. It will be recalled that the list of ten stars intentionally includes faint as well as bright stars, and those in widely different declinations.

2. It has already been the practice at the Yerkes Observatory to employ Alpha *Boötis* as a control star when the Moon or planets are not readily available; and doubtless all will gladly concur with Professor Vogel as to it and Alpha *Persei.*

3. For the sake of definiteness in carrying out the suggestions of Messrs. Bélopolsky and Deslandres, it is now proposed that the three observations of

a star be secured as follows: one plate as nearly as possible at the date of zero Earth velocity, practically at the time of the star's opposition to the Sun, and the other two plates at dates as nearly as possible thirty days before and after this date.

4. In so far as possible all will no doubt adopt the form of publication proposed by Professor Vogel. It should be said, however, that it may be somewhat inconvenient for some observers to reduce separately the measures with the plate under the microscope in the direct (readings and wave-lengths increasing) and in the reversed positions. Nevertheless, additional pains will be willingly taken for the sake of uniformity.

The early publication of previous determinations of velocities of any of the stars on the list, even if the reductions are provisional and not entirely definitive, will be of assistance in the furtherance of the scheme of coöperation. It would also appear advisable that the results that may be obtained from the observations of the "fundamental velocity stars" during the remainder of 1902 should be published as promptly as possible in 1903.

To avoid further delay, will you be kind enough to reply as promptly as possible to this letter, signifying your adhesion to the general plan here mentioned, if you feel prepared to do so, and offering such further modifications or additional suggestions as seems to you likely to contribute to the success of our common enterprise. Very respectfully, etc.

To these condensed statements from different observers should be added the following, received later, which are in reply to both circular letters:

VII. DAVID GILL (August 11, 1902): The reason why I did not reply to your circular of February 20 is that our spectograph is under alteration by the Cambridge Instrument Co. . . . . I will coöperate as far as practicable in your scheme with much pleasure whenever the spectroscope arrives, only you cannot expect us to observe the spectra of stars like α *Persei*, whose meridian zenith distance here is 83°. I should have been glad to see a greater variety in the types; the stars of your final list are nearly all of the solar type. My notion is that all observers should use the iron comparison spectrum and another comparison spectrum which should be left to the observer. Would it not be well also that at each observatory some star should be selected to be observed nearly throughout the whole year, so as to determine the mean velocity of the Earth's motion independently by spectroscopic means—in other words, to determine the solar parallax by spectroscopic methods? This should give us a tolerably sound measurement of the fundamental accuracy of our work.

VIII. H. F. NEWALL (July 25, 1902): (1) I am very glad to join in the scheme of coöperation. (2) I agree in thinking it very desirable to give the

widest latitude to each observer as to the mode of observing, reduction, choice of comparison spectrum and star lines. The more variety we have the better. (3) It seems right for a beginning to limit the list to ten bright stars. This will make a beginning for the coöperation. . . . . It would undoubtedly be a convenience to have for reference and control a much larger list of stars, about whose velocity there is something like agreement among observers. Would not the case be met by putting certain stars (say thirty or forty) on a list, inviting observers to publish any observations they may make, as soon as possible, but imposing no obligations upon an observer about making observations other than what each is satisfied with for a provisional determination (say from three plates, at any epochs)? Would it not be well to agree upon a common name by which to designate the ten stars on the list in order to facilitate indexing and references? There are objections to "fundamental" and "standard;" the word velocity can hardly be omitted. I would suggest "Velocity-Reference-Stars." (4) It is to be hoped that, for uniformity in publication, something like Professor Vogel's suggestions will be adopted. I venture to make one or two additions: (*a*) Hour-angle at mid-exposure, as well as G. M. T. (*b*) Linear dispersion in tenth-meters per mm, and in km per second per micrometer revolution. (*c*) Range of spectrum over which measurements have been made. (*d*) Comparison spectrum. (*e*) Slit-width."

The replies to the second circular letter received from the others may be condensed as follows:

A. BÉLOPOLSKY (July 17, 1902): I adopt the proposed scheme of observations of "fundamental velocity stars" at once, and will observe as rapidly as possible the following stars: α *Arietis*, α *Persei* (ι *Aurigae*), β *Geminorum* (ε *Leonis*), α *Boötis*, β *Ophiuchi*, γ *Aquilae*, ε *Pegasi*, γ *Piscis* (or γ *Cephei*). The stars in parenthesis I shall regard as supplementary stars, substitutes for those too far south for observation here. I shall observe each star three times — at opposition and about thirty days before and after opposition. In general I will adhere as closely to the program as is possible.

H. C. VOGEL (July 25, 1902): (1) I agree to the proposed list of ten stars, and (2) I am also of the opinion that the greatest possible freedom should be allowed to each observer in respect to the mode of observing, choice of comparison spectrum, selection of star lines, and method of reduction. Therefore I shall have to regard as to be recommended but not as binding the suggestion of Messrs. Bélopolsky and Deslandres that the plates be obtained thirty days before, at, and thirty days after the star's opposition. I would suggest in simplification of the form of publication I proposed, that the measurements on the plate in the two positions under the microscope need not be given separately. As a substitute for items 4 and 5 (p. 173)

read as follows: Velocity of the star in kilometers (mean of measures with violet toward right and violet toward left under the microscope), reduced to the Sun with the constants of Schlesinger's tables. To item 3 (p. 173) I would add: The data shall refer to a definitely specified line approximately in the middle of the portion of the star's spectrum measured.

E. B. FROST: Observations have been in progress with the Bruce spectrograph of the Yerkes Observatory since July, following as closely as possible the program given in the second circular letter as representing as nearly as possible the combined views of all concerned. Attention will also be given here to the supplementary stars suggested by M. Bélopolsky. α *Boötis* and α *Persei* are being frequently observed.

Communications in addition to those cited above have not been received from Messrs. Campbell, Deslandres and Lord.

It is to be hoped that the experience in this simple plan of coöperation may be satisfactory enough to lead to its further development in the future.

YERKES OBSERVATORY,
September 23, 1902.

COMET PHOTOGRAPHY WITH THE TWO-FOOT REFLECTOR.

By G. W. Ritchey.

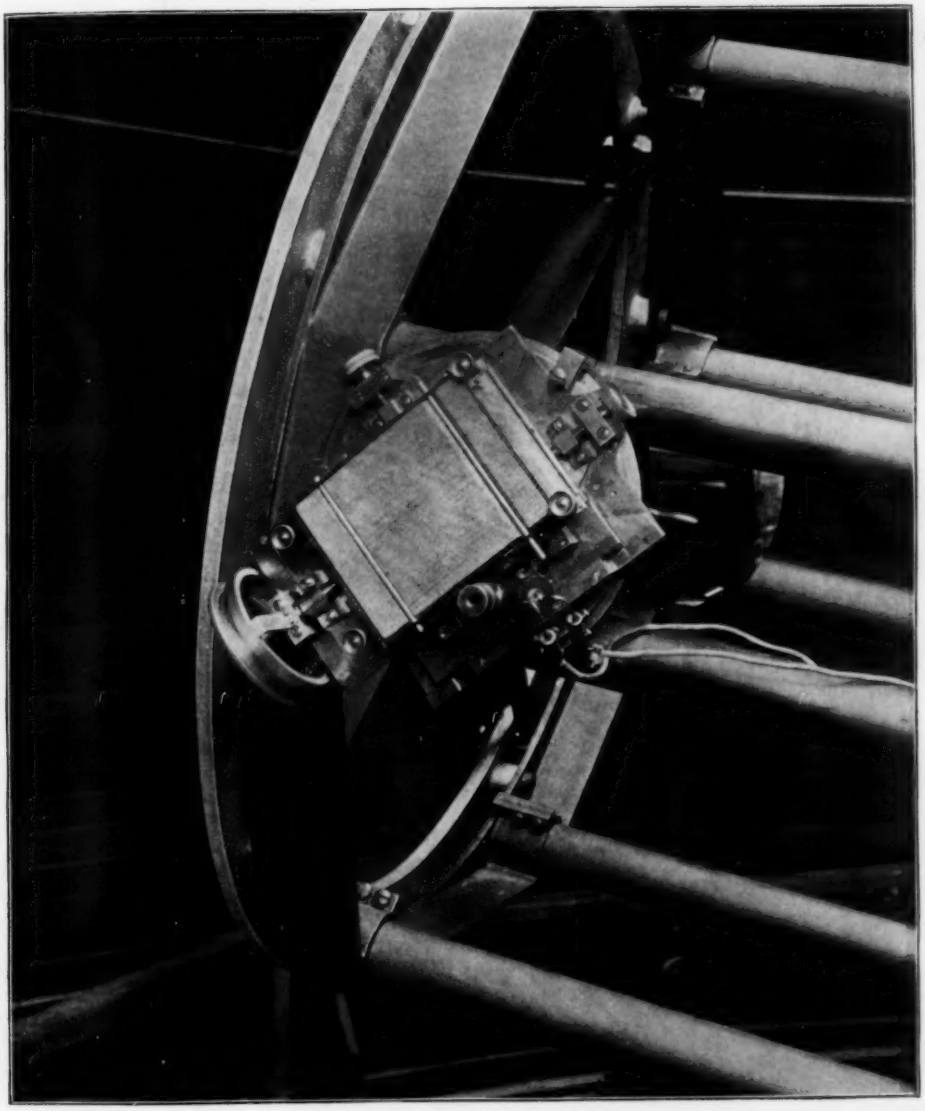
In photographing comets the usual method of guiding is by watching the nucleus of the comet in an auxiliary telescope, rigidly connected to the photographic one, and keeping this nucleus carefully bisected by the cross-wires in the eyepiece of the guiding telescope throughout the exposure, by means of the telescope's slow-motions.

In photographing stars and nebulæ the use of the double-slide plate-carrier presents many advantages over the use of a guiding telescope and the slow-motions; these advantages become more marked as larger photographic telescopes are used; indeed, for very large instruments the double-slide plate-carrier is absolutely indispensable; by its use the finest photographs yet obtained of such objects as the star-clusters and the nebulæ have been secured. This is due primarily to the fact that the incessant small corrections in guiding, the necessity for which becomes so apparent with large instruments, can be introduced with extreme accuracy and quickness. A star just outside the field being photographed is kept at the intersection of the cross-wires of the plate-carrier, by turning, when necessary, the milled heads of the fine screws which move the slides. It should be noticed that the small brilliant star image is a much better object for guiding than the usually faint and diffuse comet nucleus. But it is evident that this method of guiding is not available, without modification, in comet photography, on account of the motion of the comet with reference to the surrounding stars.

The writer, assisted by Mr. F. G. Pease, has obtained a series of photographs of Comet *b*, 1902 (Perrine), with the two-foot reflector and its double-slide plate-carrier, in the following manner.

An additional slide carrying the photographic plate was super-

PLATE VI



DOUBLE-SLIDE PLATE-CARRIER ADAPTED FOR COMET PHOTOGRAPHY

posed upon the two slides used in guiding, as shown in Plate VI. The two smaller milled heads are the ones used in keeping the guiding star exactly at the intersection of the cross-wires in the eyepiece, which is also shown. The large graduated head to the left is used to move the supplementary slide and plate-holder at such a rate and in such a direction as to compensate for the motion of the comet with reference to the star used in guiding. As will be seen in the illustration, the entire plate-carrier is rotated in position-angle to secure the proper direction of motion of the plate.

The direction and rate of motion to be given to the supplementary slide may be calculated in advance from a knowledge of the comet's orbit, or may be determined with great precision and ease by making a preparatory photograph as follows. An exposure of 30 seconds' duration is made on the comet, the observer guiding in the usual way with the double-slide plate-carrier, keeping a suitable star stationary on the cross-wires. The plate is then covered for 30 minutes, at the end of which time the guiding star is again kept at the intersection of the cross-wires while a second exposure of 30 seconds is made. Again the plate is covered for 30 minutes and a third exposure is then made. The exact durations of these exposures and of the intervals between them are carefully noted. The driving clock is now stopped and the stars in the field are allowed to trail. This preparatory plate is now developed, and is measured while still wet. The three exposures and the two intervals enable the observer to determine whether the direction and rate of the comet's motion are changing rapidly enough to effect perceptibly the sharpness of the photograph, which is made immediately after, with an exposure of an hour or more, and to allow for such changes if necessary.

Two improvements could easily be made upon the apparatus as shown in the illustration. First, instead of rotating the entire plate-carrier for position-angle as shown here, the supplementary slide only could be mounted on a plate which could be rotated. The original slides would thus remain, one in right-ascension, the

other in declination, which arrangement makes guiding on a star somewhat easier. Second, a small motor could be used to move the supplementary slide at the proper speed; as this motion need not be continuous, but can be intermittent, a very simple motor would suffice; the principal difficulty would be in regulating the speed properly, since this speed must be changed night after night. The present method of giving this motion by hand is simple and satisfactory; once in fifteen or twenty seconds the observer looks away from the guiding eyepiece, for two or three seconds only, to give the necessary movement to the graduated head; an assistant calls out the time for such movement; and to allow for the fractional parts of seconds which are generally necessary, a schedule is prepared on paper in advance, so that no cumulative error can occur during the exposure.

By these simple means the great advantages of the double-slide plate-carrier are made available for the photography of comets; and under favorable circumstances it should be possible to secure photographs of these objects as perfect in detail as the best photographs of star-clusters and nebulæ.

YERKES OBSERVATORY,
October 1, 1902.

Minor Contributions and Notes.

NOTE ON FOCUSING PRISMATIC AND GRATING CAMERAS IN ECLIPSE WORK.

Mr. W. J. Humphreys, after having discussed the spectroscopic results obtained during the solar eclipse of May 18, 1901,[1] gives some suggestions very worthy of being considered in future eclipse work. But there is one point in which I do not agree with him; and as I think I have good reasons to fear that if his advice were followed, some very remarkable phenomena would never come out so distinctly as the available instruments could show them, I am bound to submit my opinion to the notice of eclipse observers.

Mr. Humphreys says: " . . . . But since it is so essential to have exact definition, I would strongly urge final focusing, just before totality, on the narrow crescent of the Sun."

Now, I think final focusing on the spectrum of the narrow crescent must be very precarious. The Fraunhofer lines may for a few moments appear rather sharply defined, but they are very variable then and soon become faint and hazy, the spectrum of the remaining crescent of the photosphere being overlapped by the flash-spectrum and the chromospheric arcs. And focusing on the chromospheric arcs is quite impossible as long as we do not exactly know what we *ought* to see. There are at least two reasons why these arcs cannot be well-defined objects. First, the chromosphere itself is not sharply limited on the outer side, and secondly we are by no means sure that each arc, corresponding to a definite Fraunhofer line, really consists of monochromatic light. On the contrary, there is strong evidence that every chromospheric line is a narrow band with blurred edges and a dark core.[2]

As the existence of these double bands, appearing on the plates of the Dutch eclipse expedition to Sumatra, was required and foreseen by

[1] Astrophysical Journal, 15, 313-332, 1902.

[2] *Preliminary Report of the Dutch Expedition to Karang Sago (Sumatra) for the Observation of the Total Solar Eclipse of May, 1901*, by W. H. Julius, J. H. Wilterdink, and A. A. Nyland, Amsterdam, 1902.

a theory covering a great many solar phenomena,[1] it is an important question to ascertain whether the duplicate is real or not, and future observers should no longer bid fair to miss it by focusing, as may often have happened.

Indeed, there is an almost general complaint to be found in the eclipse literature, that the plates appeared not to have been exactly in focus.

In those cases, however, where the focusing had been very carefully carried out beforehand, the want of distinctness on the eclipse plates may just have revealed the true nature of the spectrum; but most observers will not have accepted this result, believing that the chromospheric lines ought to appear nearly monochromatic, and they have, of course, preferred to focus on the phenomenon itself until the arcs came out as single as possible.

The above considerations induce me decidedly to advise focusing by some method independent of the eclipse phenomena.

In case a perfectly reliable collimator should not be available, star spectra should be used. (At Karang Sago Professor Nyland focused on the spectrums of Arcturus the night before the eclipse.) It may happen, however, that considerable differences in the temperature at night and day time, or other circumstances, make it desirable to control the focus a few hours before totality. Then the following device is recommended, which is so simple that it cannot be new.

At a considerable distance from the grating or prism-camera (say twenty-five or fifty times the focal length) a large black screen with a slit of proper dimensions is erected, in a convenient direction, such that the siderostat mirror may be so adjusted as to reflect the light from the slit into the instrument. The slit may be illuminated by means of a plane mirror placed behind the screen and reflecting a bright part of the sky or direct sunlight. Some more black screens with openings on the path of the beam will do for sufficiently excluding extraneous light.

Let the distance along the beam between the slit and the objective be p.

Now the Fraunhofer lines are made to appear as sharp as possible in the plane of the cross-wires of the ocular or on the photographic

[1] W. H. JULIUS, "On the Origin of Double Lines in the Spectrum of the Chromosphere, Due to Anomalous Dispersion of the Light from the Photosphere." ASTROPHYSICAL JOURNAL, **15**, 28–37, 1902.

plate. Let the distance between this conjugate focus and the second nodal point of the objective be called q. Then the focal length is

$$f = \frac{pq}{p+q},$$

so that, in order to bring the wires or the plate exactly in the principal focus, we only have to move them toward the lens over a distance

$$d = q - f = \frac{q^2}{p+q}.$$

This is easily done by means of a short scale and vernier on the sliding tubes, provided the value of the quotient be known with sufficient accuracy. Now, for measuring the long distance p, a surveyor's chain gives ample exactness; and if we have taken p about fifty times as long as q, d is of the order of magnitude $\frac{q}{51}$. Consequently a slight error in the evaluation of q has an insignificant influence on d.

W. H. JULIUS.

UTRECHT, August 1902.

ON THE CHANGE IN THE FOCUS FOR *NOVA PERSEI.*

In the ASTROPHYSICAL JOURNAL for October 1901 (**14**, 151–157), and in *Astronomische Nachrichten*, **159**, 49–58, 1902, I have given some measures for the focus of *Nova Persei* compared with that for a fixed star. It was shown in these papers that there was no difference whatever between the focus for the *Nova* and that for an ordinary star. The spectrum of the *Nova* showed the nebular lines, and it was thought that there might be a difference in its focus, like that shown in the planetary nebulæ (ASTROPHYSICAL JOURNAL, *loc. cit.*), where the focus is ¼ inch (6 mm) greater than for a star.

Here are the results of these tests for focus:

				Scale reading in.	
1901	August	12	*Nova* - - -	2.25	(4 obs.)
			B.D. 43°.732 - -	2.26	(4 obs.)
			Nova − 43°.732 =	− 0.01	(0.25 mm)
	September	3	*Nova* - - -	2.29	(5 obs.)
			B.D. 43°.732 - -	2.30	(5 obs.)
			Nova − 43°.732 =	− 0.01	

				Scale reading in.	
1902	January	31	*Nova* - - -	2.10	(5 obs.)
			B.D. 43°732 - -	2.11	(5 obs.)
			Nova − 43°732 =	− 0.01	
	January	31	*Nova* - - -	2.10	(5 obs.)
			B.D. 43°720 - -	2.12	(5 obs.)
			Nova − 43°720 =	− 0.02 (0.5 mm)	

Powers of from 700 to 1300 diameters were used in these tests. The persistent minus sign (showing a shorter focus for the *Nova*) is doubtless accidental.

It was, however, suggested that an increase in focus might present itself later on in the case of this star. Since then a careful watch has been kept to see when the phenomenon occurred, if it should occur.

This change has now taken place.

After its emergence from the region of the Sun this year, the *Nova* has been observed since July 14. The star had faded considerably and has since faded still further, until it is now but slightly brighter than the tenth magnitude. No change in its focus seemed to have taken place in the meantime.

The note-book says on July 14: "The *Nova* is bluish white; there does not seem to be any difference of focus." No scale readings.

On August 29 careful measures for focus were made of the *Nova* and the star *B.D.* 43°739, with the following result:

Scale reading for focus of *Nova* -	2.31 inches	(3 obs.)
Scale reading for focus of 43°739 -	2.29 inches	(5 obs.)
Nova − 43°739 =	+ 0.02	

This difference was so small that it was not considered evidence of change.

The note accompanying these measures says: "The *Nova* has a pale whitish color, and it does not seem so well defined as the star."

This lack of any decided color in the *Nova* has been present for some time since then; previously it had been greenish or bluish-white.

Following are some of the subsequent notes:

1902 September 1. It is a pale whitish color.
8. The light is whitish with suggestion of greenish tinge.
9. It has a pale whitish color.
15. The *Nova* is of a whitish color, with a slight bluish-green cast.

1902 September 16. The *Nova* is whitish and dim.

18. It has a pale white color.

29. The *Nova* is dull and bluish-white. In the finder it does not appear so sharp as the comparison star, and looks slightly hazy at best.

Observations on the following date show that the color seems to have changed as well as the focus:

October 6. The *Nova* is very bluish-white. It is bluer than usual. When the star is in focus the *Nova* is out of focus, and has a small point of light in the center—somewhat resembling *Nova Aurigae* in the latter part of 1892.

With a power of 700, the focus for the *Nova* and for the star *B.D.* 43°.739 was very carefully determined. The difference is very decided. Similar measures were made on October 7 and 13.

The results are as follows:

	Oct. 6 In.		Oct. 7 In.		Oct. 13 In.	
Focus for *Nova*	2.43	(8 obs.)	2.40	(6 obs.)	2.34	(5 obs.)
Focus for star	2.19	(8 obs.)	2.21	(6 obs.)	2.12	(5 obs.)
Focus for *Nova*—focus for star	+0.24	(6.1 mm)	+0.19	(4.8 mm)	+0.22	(5.6 mm)

Mean for the three nights, +0.22 inches (5.6 mm)

The focus has therefore changed to correspond with that of the planetary nebulæ, and is now about 0.2 inch greater than that for a star. This change has been very recent—certainly since August 29, and doubtless within a much shorter interval than that, as I think I should have noticed a difference of anything like this amount in the frequent examinations of the star.

E. E. Barnard.

Yerkes Observatory,
October 7, 1902.

NOTICE.

The scope of the ASTROPHYSICAL JOURNAL includes all investigations of radiant energy, whether conducted in the observatory or in the laboratory. The subjects to which special attention will be given are photographic and visual observations of the heavenly bodies (other than those pertaining to "astronomy of position"); spectroscopic, photometric, bolometric, and radiometric work of all kinds; descriptions of instruments and apparatus used in such investigations; and theoretical papers bearing on any of these subjects.

In the department of *Minor Contributions and Notes* subjects may be discussed which belong to other closely related fields of investigation.

Articles written in any language will be accepted for publication, but unless a wish to the contrary is expressed by the author, they will be translated into English. Tables of wave-lengths will be printed with the short wave-lengths at the top, and maps of spectra with the red end on the right, unless the author requests that the reverse procedure be followed.

Authors are particularly requested to uniformly employ the metric units of length and mass; the English equivalents may be added if desired.

If a request is sent *with the manuscript* one hundred reprint copies of each paper, bound in covers, will be furnished free of charge to the author. Additional copies may be obtained at cost price. No reprints can be sent unless a request for them is received before the JOURNAL goes to press.

The editors do not hold themselves responsible for opinions expressed by contributors.

The ASTROPHYSICAL JOURNAL is published monthly except in February and August. The annual subscription price for the United States, Canada, and Mexico is $4.00; for other countries in the Postal Union it is 18 shillings, 6 pence. Correspondence relating to subscriptions and advertisements should be addressed to *The University of Chicago, University Press Division, Chicago, Ill.*

Wm. Wesley & Sons, 28 Essex St., Strand, London, are sole European agents, and to them all European subscriptions should be addressed.

All papers for publication and correspondence relating to contributions and exchanges should be addressed to *Editors of the* ASTROPHYSICAL JOURNAL, *Yerkes Observatory, Williams Bay, Wisconsin, U. S. A.*